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A CASE OF COLOR-BLINDNESS TO YELLOW AND TO BLUE[1]

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In November, 1909, a member of a class in elementary psychology in Drake University had difficulty in obtaining afterimages. The instructor of the class, the writer of this report, tested him with the Holmgren wools. He matched the red skein normally and quickly, found more slowly a few matches for the green, but these were good, and matched the purple with the light reds and pinks. Upon being questioned he answered that the two skeins (the red and the purple) were of the same color; that one (the purple) was a "thin" red, and the other a "good" red; but that so far as mere color was concerned, any skein that would match the one would match the other. It was here evident that the admixture of blue in the purple skein did not to him affect the color. He was then given a skein approximating a standard blue. This he matched hesitatingly with light yellows, light greens, light browns, light greys and other blues of the lighter tints. A yellow skein was matched with the darker of the light greens, greys, green-blues and yellows. Being further questioned, he stated that the two skeins (the blue and the yellow) were alike except as to brightness, one (the blue) being much brighter than the other, but that neither had color. He suspected from the texture that they might be colored, but to him they appeared uncolored.

It was evident that this was, at least, an unusual case of color-vision and further investigation was carried on as faithfully as possible for the remainder of his college course—a period

[1]A preliminary report of the case was made at the meeting of the American Psychological Association at Minneapolis, Minn. December, 1910.

The writer will be glad to put any one interested into communication with the subject of the investigation.

of somewhat more than two years—and through portions of the summer of 1911. Several weeks were spent at the University of Chicago and at the University of Nebraska, using such apparatus as was available at the psychological and physical laboratories of these institutions.

Beginning June 15th, 1914, more than four and one-half years after the first observations on the case, the subject was given a brief series of tests paralleling those already carried out. The purpose of these was to determine whether there was any evidence of deterioration in his vision, as this might have an important bearing on the question whether the case is of pathological or congenital origin. His reactions in this series showed no sign whatever of any change for the worse in his color judgments, and in several tests his color discriminations were more accurate than they had been in the earlier experimentation. His eyes were in excellent condition, apparently in every respect better than when he was in school. During the past year he had been teaching chemistry and physics in a small High School. He reported that during the winter he had less trouble than usual in strong light, and that the glare of sunlight upon snow had not been annoying enough to necessitate the use of colored glasses.

A letter from him in 1916 indicated a suspicion on his part that he could not distinguish colors as readily as when he was in school. He complained that color now "wanders like a drunken night." He was sent the set of colored disks with instructions to sort out the colored, the uncolored, and the greys. He returned them with the assortment made as usual. In the summer of 1921 the writer was again in communication with him by letter, and he was then quite sure that his vision, both as to color as to acuity, was undergoing a change for the worse. Much of the color, he said, seemed to have gone out of the world. However, he chose the colored, uncolored, and grey disks practically as he did when in college. He, of course, made these selections entirely alone, and said that they were made on the basis of *color*. He mentioned also a grey film that sometimes came over the field of vision of his left eye.

The young man, who will be referred to hereafter as D, was about twenty-four years old at the time the case was first noticed. He was a member of the Drake University football team for the three last years of his college attendance. An ophthalmological examination[2] showed the retina to be normal, acuity normal, and no coloration of the lens. The color-blindness was apparently not pathological, at least so far as gross examination of the retinas would indicate.

[2]This examination was made at different times in December, 1909, by Dr. W. W. Pearson of Des Moines.

The hereditary features of the case are interesting. There is color-blindness in the immediately preceding generation on both sides of the house. The subject's father and mother were found (twenty years after marriage) to be second cousins. This seems to be good evidence that the color-blindness is congenital.

An exhaustive examination of this case presented a number of difficulties. D's retinas are apparently sensitive to slight differences in degree of brightness to which the normal eye is not sensitive. This very great susceptibility to slight differences means a most unusual sensitivity to changes in general illumination which makes the work exceedingly difficult to control. Color stimuli in which color was sensed in a certain degree of illumination lost color completely with a slightly increased illumination. On several occasions, when D was matching colors on a color-wheel in north light, the disks then appearing to him colorless, a cloud obscured the sun, and he exclaimed that there was color in the disks. The reverse of this happened several times.

The state of adaptation of his retinas, due to general light conditions or to immediate pre-exposure, was also a continual source of variation in the results. D's daily variation in general physical condition depending upon lack of sleep or anxiety, as well as upon fatigue of the eyes following study, or diversion, or prolonged exposure to out-of-door illumination, greatly affected his ability to see and to discriminate color. The fact, too, that fatigue of the retinas after a very few judgments greatly raised the threshold of color sensitivity made short periods and few consecutive trials a necessity. D, during the entire series of tests, was exceedingly generous in both time and patience, yet with school-work, athletics, and the necessity of financing his college course, could not give an unlimited amount of time.

The history of the case as written by D follows.

"The only case of color-blindness of any note that I find in my father's family is Father himself. The first time I learned of the fact was at a dinner party when Father lost his temper because one of the ladies told him there was a green stripe in his new suit. I learned from Mother afterward that it was always a sore spot with Father, and that if she wanted anything from a drygoods store she always sent a sample along.

"In my Mother's family there are a number of well defined cases. Of the 28 replies to inquiries that I sent out there were seven different cases spoken of. One, that of an uncle, was spoken of in nearly every letter, and it was a sort of a joke in that he was reported to have seldom been seen with a mated pair of socks. He seemed to have no idea concerning either shade or color, and this was especially so in the case of fruit. He was probably totally color blind. Another case was spoken of a number of times, a lady cousin unable to distinguish red and green. Another cousin wrote me that he could see colors if they were not too bright, but if they were very bright they all looked white. Another cousin wrote that he never knew he was color blind until he took a railroad examination, but he thinks he sees color as well as any one. Of the others I remember very little of what they had to say. Besides the seven mentioned there were a

number of others that admitted having difficulty with colors, but seemed unwilling to discuss it. Of the seven, four were men.

"A very peculiar condition existed in the case of one of my uncles who was not color blind, but lost his sight entirely for about twelve years. He recovered it in his seventy-eighth year without medical aid.

"My Father's people are German; all large and strong, generally of light complexion with blue eyes. Mother's people are Swiss; small of stature, with dark hair and eyes. Both families, however, are long-lived and very healthy. Both families were members of the Penn colony, but migrated to Somerset County, Pa., before the Revolution. Numbers of them were killed in Indian wars.

"I was born in Iowa in February, 1886, the second of a family of four, an older sister and a brother and sister younger. Father was a farmer, as were three of his brothers also, one brother being a minister and the other a lawyer. There were also six boys and four girls in my Mother's family, four ministers, four farmers, and a dentist and a physician.

"I learned when quite young to consider color-blindness as a great defect, partly because Father often made unfortunate mistakes.

"The first difficulty that I can remember was to find out what people meant when they talked of the sky as blue. I remember when I thought that an apple must be red to be ripe, and I was perhaps six years old before I learned to tell by touch a ripe fruit, and even then I preferred it red. At home we raised three kinds of corn; red, yellow, and white. Red was red, white was smooth, and yellow was rough, and it was years before I could convince myself that a thing could be yellow, and not rough. White corn I could distinguish from yellow only by the shape of the ear, and the kernel. They looked alike in color. Questions often arose, but I dared not ask lest I expose my ignorance. I tried to solve the problem by remembering how a thing looked and felt that was spoken of as colored other than red, and by comparing texture and shade I experienced little difficulty under ordinary circumstances. The only difference between fresh cut grass and the bleached product was that the latter had lost its brilliancy. Standing grass was darker and had a surface that I associated with many other things people called green, though many of the things spoken of as green were to me nearly white. I could not understand why people spoke of a *golden* field of grain and a *green* hayfield, for they looked very much alike to me.

"Paints I soon learned to distinguish, and seldom if ever get them confused as no two have the same kind of surface unless highly polished. Even blue in paints sometimes has texture, though in other things it is usually so bright that I am unable to distinguish anything in its surface.

"At the age of fifteen I left the farm for a small academy and was obliged to work my own way. Railroading suggested itself to me as "easy money," and to prepare for the examination I procured pieces of colored glass commonly used for signal lights and signboards, and set about studying them. I found that the texture of the glass was not the same as that of paint or of cloth. So I procured spools of silk thread and a sample card of house paints, and studied them. I took the examination and passed with but one or two minor errors. I went to work thinking I could tell colors as well as any one, but the eighth day I bumped up against the colored lights alone, ran a switch engine into a box car, and concluded that it was no place for me.

"Later when I entered college I found a complete change of conditions and it was here that I encountered my first real difficulties. I was working my way, which was at times a severe tax on my system, and the first place to be attacked was my eyes, though only twice during my college work was my sight badly impaired for study, and that followed continued loss of sleep.

"I took up chemistry as a freshman science and it was not long until I wished that I had not; however, I had started it and had too much *Dutch* blood to quit. I soon found that there are other characteristics just as distinctive as color. There were colored salts, solutions, flames and precipitates. Flames were easy as no two things volatilize at the same temperatures and their texture is entirely different. Copper and barium are both green, but otherwise are not nearly so much alike as barium and strontium, the barium being very soft and the copper hard, almost flinty.

"Precipitates were not so bad unless in a colored solution as they all have a definite texture as well as a time for precipitation. Salts are not so important as far as color goes, but as for solutions I have never been able to find a way of telling their colors that was reliable, principally because they are transparent and one cannot see their surface.

"My plan was always where possible to note other characteristics than those of color, and as far as I know I have been just as successful in analytical chemistry as the rest of the class. I can see red in very small amounts (except in very strong light) better, I am sure, than most people, and green in dark shades in dull light. Blue is strong white with a glint to it, and yellow is usually a dull grey."

Tests With The Holmgren Wools

Further tests were made with the Holmgren wools,[3] which were spread upon a table with a dull black painted surface.

D's choices in this test were in the main *very* consistent, except in the case of a group of light tints about which he was always doubtful. The following record of one test is typical of the results:

101 test skein (green): selected skeins numbering 5, 11, 13, 15, 19, 43, 45, 47, 49, 51, 53, 55, 57, 59, 63, 67, 69, 97, 99. (These are all greens—see footnote—as are the doubtful skeins.) Doubtful: 1, 3, 7, 9, 17, 41, 61, 65.

Observations:

"Didn't notice green (in 101) at first glance; it came all at once in a flash. I could easily have been mistaken in it."

"These" (7, 53, and 3) "I first thought were green"; then he threw 53 into the pile of greens, and 7 and 3 into the heap of doubtfuls.

"This one (41) I'm dubious about"; then added, "if those others" (indicating doubtfuls) "are green this one is." "This probably isn't legitimate, but I'm picking these last ones out by elimination of the ones I'm sure aren't green"; then he tossed 11, 61, and 99 to the pile of greens.

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[3]The wools are tagged by number from 1 to 103 inclusive, and by the letters of the alphabet. The even numbers from 2 to 20, and from 32 to 68 (the designations are all inclusive), are *neutrals*,—*greys* and *browns*. The odd numbers from 1 to 19 and from 41 to 69 are *greens*. The odd numbers from 21 to 29 are *violets*, *purples* and *reds*, and from 31 to 39 are *reds* and *oranges*; the odd numbers from 71 to 93 are *pinks* and *reds*. The even numbers from 22 to 30 (except 28, which is violet) and from 70 to 78 are *blues*, and from 80 to 93 are violets and purples. 94, 96, 98, and 100 are dark grey, tan, tan and brown, respectively; 95 is dark red-brown, 97 is light green, and 99 is medium blue-green. 101, 102, and 103 are the green, purple and red test skeins. (Whipple speaks of 102 as rose.) Of those designated by letters of the alphabet, A to L are different tones of browns, as are N and P. R and Z are yellowish tints.

Questioned as to his method of choice, he remarked that if he placed the doubtful skein among light skeins he could see color then if ever. No. 61 was to him green among light skeins, and colorless among dark ones. Given a sheet of white paper he compared the appearance of some of the doubtful skeins against the white and the black background. All of them that were colorless on black were green on a white ground.

He chose 19 as the best green.

103 test skein (red): selected skeins numbering 21, 23, 25, 27, 29, 31, 33, 35, 71, 73, 75, 77, 79, 81, 83, 85, 86, 87, 89, 91, 93, 98, 105, X, and V.

Doubtful: 80, 88, 90, 92, "probably red." These are orange, pink, violet, purple and red.

Looked at 37 (deep orange) in different lights. Being questioned replied, "It's that bad one." (He has occasionally decided "it has red in it.") He said that 37 most nearly matches 80 (a *light* light violet) except that 80 has "lustre" and 37 is a "dull, dead surface."

92 (a dark violet) most nearly matches 37 in *color* (but not in brightness); it (37) "can't be matched in shade as it is darker than any of them."

When asked if he suspected any of the skeins of being yellow, he selected 37, O (a strong light yellow-brown), and 56 (a tan). He chose these, he said, "because of the mixture; yellow has some red texture to it, and also some green.[4] Red on green will darken a white; get one dulled down in shade and I'm suspicious that it might be yellow. I figure that if it weren't so dull it wouldn't be yellow. I can figure blues easier because of the brilliant surface. Two ways of getting blue: pick out the *brightest* in the bunch and call it blue, or hunt for a *mixture* that acts right." For blues, he selected 78 (dark blue), 39 (orange-yellow), and O (light blue-violet). 24 (a dark light-blue) and 69 (a dark yellow-green) might be either blue or yellow. 69 he sometimes selected as green and sometimes refused as green. He *always* studied it.

On several occasions D selected the whites, the greys and the browns. Brown he characterized as a mixture of red and grey, so mixed "that it isn't red any more." He selects browns rapidly. Here is a typical assortment: 10, 12, 32, 34, 36, 38, 48, 60, 66, 68, 94, 95, 100, A, B, C, D, P, and N. These are browns. His selection of greys, which he makes rapidly, is typified by this list: 3, 5, 8, 11, 15, 20, 42, 46, 56, 58, 62, 63, 64, 65, 67, 99, 101, E, F, G, K, and L. He later added 44, 54, 96 and S. These are browns and greens.

As he referred to certain "colorless" skeins in answering inquiries, he made this assortment of those he considered colorless. This was done rapidly: 4, 16, 18, 26, 30, 41, 70, 98, Q, R, T, W, X, Y, and Z. "Some of these are dirty whites, that look as if soap and water would help, especially these (16, 36, and Y)." (This was a *new* box of wools.) He often referred to the "dirty look" of yellowish skeins. The following collection remained after the above selections were made, and he could only characterize them as not white, nor grey, "just leftovers": 1, 2, 6, 7, 9, 14, 17, 22, 40, 43, 50, 52, 57, 61, 72, 97, H, and M. (These were light greens, blues, yellows and tans, but no light pinks, reds, nor violets.)

In short, the Holmgren test afforded him no difficulty with reds, except that the violets and purples were added as reds; he had difficulty with light greens, and with darker greens tinged

[4] He will insist occasionally that a certain green under consideration has a "reddish cast."

with yellow or blue. Blues, yellows, and light tints—except reds—were confusing.

Colored Papers

Given the assortment of Hering colored disks and asked to choose the colored ones, D always immediately selected the reds,[5] violet and purple. If the day were dull, or the illumination of the room reduced, his eyes in good condition, and his attention keen, he could be relied upon to choose the green, and often the blue-green and yellow-green. Very often he would hesitate over the orange. If questioned, he would reply that in "certain lights" he suspected it of having color. Asked *what* color, he would always answer, "Red, if any." He was apparently never inclined to think that it might be green.[6]

The yellow disk was never judged as having color, and the reddish yellow was sometimes said to have the *texture* of color but not color. If he were asked *what* color he would guess if he must guess, he always said red, as it was rather 'darkish' for a green. The yellow was to him a colorless, or a grey surface.

The green-blue and the blue were not only colorless, but *brilliant*. On several occasions he referred to the surface of the one (the greenish-blue) as "enamelled," and that of the other (the blue) as having "two coats" (of enamel). The (blue) surface had a high lustre to it, "little glints all through it," "a gleam that goes with what people call blue."[7]

After a period of over two years (1914), in which he had not seen Hering papers, the series was laid before him and he was asked if he remembered any of them especially. He picked up the

[5]The Hering papers were of the second series, purchased from C. H. Stoelting in the winter of 1908, and were named at that time as follows: red, nos. 1 and 2, orange (Gelb-Rot), reddish yellow (Rötlich Gelb), yellow, yellow-green (Gelb-Grün), green, blue-green (Blau-Grün), greenish blue (Grünlich Blau), blue, violet (Rötlich Blau), and purple (Blau-Rot). The red of the following equations is no. 1.

[6]In fact, in all the notes on his observations, there is but one record of his confusing red and green, and in that instance he judged a green coming into the periphery as "Colored, don't know what yet"; as it came in, he added, "It's green—no, it isn't, it is red!" This confusion was doubtless due to the negative after-image, and was probably not an instance of *color*-confusion.

[7]D says that some blue ink is hard to read: for instance, Sanford's Premium Writing Fluid before turning black is hardly visible on white paper. A blue pencil makes a mark scarcely different from that of white chalk. A blue ribbon on a type-writer gives the same effect on paper as the keys hitting the paper direct without a ribbon, and can be read only with the paper "on a slant." To try out one of these observations, the experimenter put before D a sheet of white paper upon which she had written a message with a blue pencil. D did not notice the message for some time, though he was using the paper for a background. When his attention was called to the note, he tilted the paper downward toward the window and read it easily.

orange and answered, "Indeed, I do. That's the worst surface I ever saw, and some day I should like to know what it is." Holding it at different angles, he commented on the fact that it had texture of color, and the shade of color, and occasionally a suspicion of red, "but it (the color) doesn't stay long enough for me to look at it."

He had no hesitancy at this time in choosing the colored disks, laying out the reds, yellow-green, green, blue-green, violet and purple, and putting aside the orange which he regarded with misgivings.

In 1921 he designated as reds, in each of eight trials on different days, the two reds and the violet; the purple as red seven times and grey once; the green as "probably green though light;" the yellow-green as "green at first" once, the other times grey; the orange as "some color—not grey," "colored?," "can't say," and five times as grey; the reddish-yellow as grey, nearly white, or white; the yellow as grey, or white; and the greenish-blue and the blue as white.

Spectrum Tests

Professor D. W. Morehouse of the Physics Laboratory of Drake University, in December, 1909, first tested D's ability to discriminate colors in the spectrum, using a sunlight source, and Rowland's grating ruled 14438 lines to the inch, mounted on a nine-inch telescope. The writer had asked for only a rough determination, so that the wave-lengths were not given. In this, as in subsequent examinations with the spectrum, the method of color-naming was used. Professor Morehouse made the following statement:

"The infra edge of the red began at a very low point in the spectrum of the first order, where the violet of the second order overlaps the red of the first.

"The red-yellow band, for him, was about midway between the C and B lines. This is very low indeed; the orange does not begin this low.

"The spectrum was colorless from the D lines almost to the H lines where the red began again.

"The brightest portion of the spectrum was in the green or at the b lines. It was not green for him, but very bright."[8]

The next attempts[9] at accurate determinations of the limits of the visible spectrum as seen by D were made with an ordinary

[8]The apparent inconsistency between this and later determinations is due to the intensity of the beam used as a source. In such a case the green has always been obliterated, and the brightness determinations have been exceedingly variable, because, as D once remarked, "When it (the green-blue portion of the spectrum) is so bright, I can't tell *where* it is brightest. It is all glare, and it hurts!"

In no case, after this was noted, was it impossible to locate the green band when the slit was sufficiently narrowed.

[9]These were made by Mr. Seth Nicholson, then of the Physics Laboratory of Drake University.

laboratory spectroscope, using a 16 c.p. incandescent electric lamp as the source of light. The spectroscope was fitted up with a Wallace replica of Rowland's diffraction grating of 14438 lines to the inch. The right eye alone was used in these tests and it was light-adapted throughout. Under these conditions the subject did not observe the green band of the spectrum. The readings obtained, therefore, refer to the limits of the red band at the infra end of the spectrum, and those of the violet at the ultra end. The limits, also, afford the boundaries of the neutral band, which in this case extends from the inner margin of the red (somewhere in the vicinity of the D lines) to the inner margin of the violet (in the neighborhood of the G line.)

The mean of five readings gave the limits of the red band as 7128 A — 5896 A[10]; the second red zone (in the violet), 4442 A — 4066 A. This, obviously, fixes the limits of the intermediate neutral band, under *these* conditions, at 5896 A — 4442 A.

The loss of the green band was again due to the brilliance of the beam used as a source. This corroborates the statement of the tendency to obliteration of green in strong illumination.

Through the kindness of Professor Henry G. Gale of the Department of Physics of the University of Chicago, and with his assistance, a very painstaking series of observations was made with the use of the Rowland concave grating there installed. This is a 6-inch grating of a 21-foot radius. Sunlight was the source of light, and the tests were carried on in a darkened room.

The limits obtained under these conditions were: red, 7620 A — 6050 A; neutral zone, 6050 A — 5600 A; green, 5600 A — 4920 A; neutral, 4920 A — 4575 A; red band at violet end, 4575 A — 3933 A.

Several interesting features presented themselves during the course of the tests. Excerpts from the notes, as taken, are appended in order to call attention, briefly, to these various points in their appropriate settings. According to the usual procedure, the slit was set at certain known lines, and the observations taken:

6563 A — "good red."
6868 A — "good red."
7620 A — "still a good red, but not so much of it."
7500 A — "trace of red."
7400 A — "seems red" at times and at times "colorless."
5890 A —
5896 A — } (D lines) no color.
6050 A — little red scattered in the white, but it goes at once.
5580 A — trace of green. (Have to narrow slit to get color. With wider slit, get a colorless backgroud and green lines: with narrower slit get green background and relatively black lines.)

[10]The spectrum readings are given throughout in Angström units.

5600 A — corrected limit for green; judged one side as slightly greenish, the other side as white light. Cut down slit at magnesium lines, 5175 A, called lines green but background nearly white.
4920 A — possible trace of green.
4900 A — no color.
4565 A — "little smear of thin red."
4535 A — "still red."
4560 A — does not see it.
4575 A — lines reddish on colorless ground; background becomes red when slit is narrowed.
3968 A — "red."
3933 A — red *lines*, but no color in background.

The following observations were made with a mercury arc spectrum:

5790 A — and 5769 A; light grey.
5460 A — white.
4916 A — white.
4358 A — white, same shade as the second one before this (5460 A), but very brilliant.
4078 A and 4047 A — both red lines.

In the arc light spectrum (made with a spectroscope constructed in Ryerson Laboratory) the lines at 3850 A were examined. These for D were red. Immediately after this test, Professor Gale remarked, "He sees violet better than the normal eye; he has an excellent ultra-violet eye." The brightest point in the entire spectrum (with the carbon arc) was at 4300 A. It was white and most brilliant. This line for the normal eye is in the blue band. In the potassium spectrum D saw red unmistakably at 7699 A, and it could not be determined how much farther out he might perceive red.

The limits of the visible spectrum are given by Baly as from about 7600 to 3600 Angström units,[11] and by Helmholtz[12] as from 7604 A to 3968 A. So it is evident that there is at least no *shortening* of the spectrum. On the contrary there is some evidence that there is an extension of the spectrum at both the red and the violet ends.

The limits of the two neutral bands are given only on the basis of readings taken with the normal grating-spectrum, rather than upon the basis of readings taken with the prism spectrum. These limits of the neutral band, given as the region *between* the limits of the colored bands, are based upon Professor Gale's reading with the sunlight grating-spectrum. The first neutral band

[11]Baly, Spectroscopy, 30.

[12]Helmholtz, Physiol. Optik, quoted by Parsons, Color Vision, 28.

(normally yellow) extends from 6050 A to 5600 A; the second (normally blue) extends from 4920 A to 4575 A.[18]

In order to observe the consistency in D's color judgments, a more extensive set of readings of the limits of the colored bands was carried on in the Physics Laboratory of the University of Nebraska, using the Feuss monochromator set up with a Pellin and Broca prism, 3° dispersion. By means of a calibrated cylinder, the wave lengths may be read direct. Four different sources of light were used: sunlight, diffuse north daylight, a Nernst filament, and an arc lamp. The readings given below are for the right eye. The left eye was given the same number and kind of tests, but the only differences in the sensitivity of the eyes were irregular and less than the variations due to changes in illumination and fatigue, so that no constant noticeable difference in the sensitivity of the two eyes was observed.

These readings from the prism spectrum are not so reliable as those from the normal spectrum on account of the dispersion of the prism, which contracts the width of the colored bands through the middle of the spectrum. Therefore, their greatest value as indicated above lies in the fact that, owing to the number of readings obtained, they afford opportunity to estimate the degree of uniformity in D's judgments at different portions of the spectrum. Any other deductions from the readings are merely corroborative of phases otherwhere noted.

The following tables give the readings taken with the monochromator using the different sources of light as indicated.

TABLE I. READINGS OF RED BAND

NERNST FILAMENT	SUNLIGHT	DIFFUSE DAYLIGHT	ARC LAMP
7750 A – 5900 A	7550 A – 5900 A	7100 A – 5900 A	7750 A – 5890 A
7750 A – 5900 A	7750 A – 5900 A	7170 A – 5920 A	7750 A – 5900 A
7750 A – 5890 A	7750 A – 5920 A	7630 A – 5900 A	7750 A – 5900 A
7750 A – 5890 A	7310 A – 5940 A	7750 A – 5920 A	7750 A – 5910 A
7750 A – 5900 A	7470 A – 5920 A	7750 A – 5910 A	7750 A – 5890 A
		7750 A – 5900 A	
		7750 A – 5900 A	
		7690 A – 5930 A	
		7470 A – 5900 A	
		7750 A – 5890 A	

[18] The averages of 70 people with normal color vision (Burch) give the extent of the color bands as:

red	= 760.6	to 551	mm.
green	= 593	to 484	mm.
blue	= 517	to 443	mm.
violet	= 454	to 397.5	mm.

pure red	= 625 mm.
pure green	= 525 mm.
pure blue	= 471 mm.
pure violet	= 415 mm.

Quoted by Parsons, *ibid.*, 114-115.

TABLE II. READINGS OF GREEN BAND

NERNST FILAMENT	SUNLIGHT	DIFFUSE DAYLIGHT	ARC LAMP
5760 A—4900 A	5700 A—4960 A	5690 A—4800 A	5660 A—4820 A
5750 A—4890 A	5740 A—4980 A	5810 A—4900 A	5600 A—4810 A
5630 A—4870 A	5760 A—4920 A	5770 A—4840 A	5620 A—4810 A
5600 A—4860 A	5750 A—4860 A	5770 A—4860 A	5690 A—4870 A
5620 A—4880 A	5700 A—4860 A	5790 A—4940 A	5720 A—4840 A
		5750 A—4890 A	
		5760 A—4890 A	
		5780 A—4950 A	
		5730 A—4880 A	
		5690 A—4860 A	

TABLE III. READINGS OF "SECOND RED BAND" (Violet)

NERNST FILAMENT	SUNLIGHT	DIFFUSE DAYLIGHT	ARC LAMP
4680 A—3920 A	4400 A—4000 A	4750 A—4090 A	4600 A—3880 A
4770 A—4050 A	4750 A—4000 A	4780 A—4050 A	4650 A—3910 A
4650 A—4150 A	4600 A—4000 A	4780 A—4050 A	4620 A—3940 A
4640 A—4140 A	4580 A—4070 A	4800 A—3900 A	4640 A—3930 A
4660 A—4160 A	4610 A—4050 A	4770 A—4000 A	4650 A—3900 A
		4740 A—4000 A	
		4750 A—3960 A	
		4790 A—3880 A	
		4720 A—4120 A	
		4720 A—4100 A	

The consideration of the readings reveals only what might be expected, when one recalls the amount of dispersion in prism spectra. The yellow, green, and blue bands are disproportionately narrowed, and the violet band broadened. Therefore there is much less chance for variation in the middle, and a likelihood of a comparatively high variation in the extremes. This is precisely what is found to occur in the case of D's color judgments. There is a high variation in the lower edge of the red band, and particularly in both edges of the violet, or "second red band." In the case of the lower red limit taken from the spectra of the Nernst filament and of the arc lamp, there is no variation because the readings were limited by the capacity of the apparatus when D was observing a spectrum of a relatively low degree of intensity.

These low and consistent variations are a witness to the integrity and care which characterized D's work. There is no suspicion of ambiguity in D's judgments of the spectral bands. The spectrum for him is not shortened—in fact it may be slightly extended. The red band, beginning low in the spectrum, continues until it begins to merge into the yellow near the sodium lines; its limits seem to be, with prismatic spectra, under *these* conditions, about 7560 A and 5910 A with the best red at about 6670 A. The yellow band is to him colorless and of only medium intensity, and extends from about 5900 A to about 5700 A. The green band is visible in low but not in high intensity, owing

TABLE IV

Showing Readings of Bands of Grating Spectrum, and *Range* of Readings of Different Prismatic Spectrum Bands

SOURCE	RED	NEUTRAL	GREEN	NEUTRAL	RED
Sunlight (Rowland Grating)	7620A—6050A	6050A—5600A	5600A—4920A	4920A—4565A	4565A—3933A
Sunlight (Prism)	{7750A / 7310A} — {5940A / 5920A}	{5940A / 5920A} — {5760A / 5700A}	{5760A / 5700A} — {4960A / 4860A}	{4960A / 4860A} — {4750A / 4400A}	{4750A / 4400A} — {4000A / 4070A}
Nernst (Prism)	7750A — {5900A / 5890A}	{5900A / 5890A} — {5760A / 5600A}	{5760A / 5600A} — {4900A / 4860A}	{4900A / 4860A} — {4770A / 4640A}	{4770A / 4640A} — {4160A / 3920A}
Diffuse daylight (Prism)	{7750A / 7100A} — {5930A / 5890A}	{5930A / 5890A} — {5810A / 5690A}	{5810A / 5690A} — {4950A / 4800A}	{4950A / 4800A} — {4800A / 4720A}	{4800A / 4720A} — {4120A / 3880A}
Arc Lamp (Prism)	7750A — {5910A / 5890A}	{5910A / 5890A} — {5720A / 5600A}	{5720A / 5600A} — {4870A / 4810A}	{4870A / 4810A} — {4650A / 4600A}	{4650A / 4600A} — {3940A / 3880A}

partly, it seems, to the obliterating effect of brightness in the green itself. Its limits are roughly 5700 A and 4880 A, with the best green at between 5000 A and 5100 A. The second neutral band (in the blue) extends from about 4880 A to 4680 A with prismatic spectra. The second red band, or the "thin red band," runs between about 4650 A and 4010 A with the prism. This is in the violet. The blue band is of maximal brightness and no color, with the maximal luminosity at 4650 A to 4680 A. The judgment of the point of maximal brightness, like the judgment of the best green band, was an unwelcome proceeding for D on account of the painful glare of these regions, and he had not a great deal of confidence in their accuracy. The violet band is perceived as a "thin red," of the same hue as the red band, but unsaturated. This "thin red" band is often spoken of as having somewhat of the *lustre* that he notices in surfaces that to the normal eye are bluish.[14]

Color Mixtures

The color equations with colored papers are of value in a qualitative way, but are too variable to have much reliance placed upon their quantitative phase. This work suffers as does all work in color-vision investigations done with pigments. The fading of the color, even after slight use, the change in the surface due to handling, and the occasional differences in tint in new papers were all noticed and criticized by D. The variation in the amount of general illumination was an almost constant source of error. An attempt to work only at 10 o'clock in the morning on clear days was not entirely successful as a control of the illumination feature, on account of the varying density of the city atmosphere, and threatened to render the experimentation interminable. Reliable *quantitative* work must be done where the illumination can be controlled and kept constant to a chosen standard. Certainly the equations derived from work done in the shifting light of ordinary days are not to be made the basis for accurate deductions.

A rough attempt to control the amount of illumination was made by raising or lowering the curtain of the large window just behind D as he sat facing the color-mixer. Even so, the variation due to the temporary fatigue of his retinas, and to a lowered vitality, was greater than that caused by the light. On several occasions work was discontinued for several weeks while he recuperated. D insists that his color sensitivity furnishes the best indication of his general health.

[14] In 1916 arrangements were being made for further investigations at the Nela Research Laboratory, but D was unable to go to Cleveland. It may yet be possible to obtain spectrum data for a luminosity curve and for spectral color mixtures and matches.

It will be remembered that, of the set of Hering papers, D always judged the reds, violet and purple to be varying "shades" of red. The green he often reported as colored, and sometimes the blue-green and the yellow-green (v. p. 00). On several occasions he has spoken of the orange as having the texture of color, red if anything, but probably no color. This leaves the yellow, the greenish blue, and the blue as uniformly uncolored.

An alternating current motor, with a maximal speed of 5000 revolutions per minute, was used as the color mixer. In the color matching, a large uncut disk, 20 cm. in diameter, was used as a background, against which were placed the several smaller disks 11 cm. in diameter, cut to allow changes in proportions. These changes were made in such a manner that D was ignorant of their character; he did not observe the disks until the motor was in full operation.

The discussions of the equations will be taken up in order.

(1) *Red.* D strongly opposed any attempt to match red with any disk or combination of disks not containing red, and could not be deceived in such an attempt. An increment of 12° of white, or 5° of black in the inner red disk was noticed by him as a difference in the amount of red in the inner, as compared with the outer disk.

While no combination could be made to match 360° of red, a mixture of red with black or white or both could be matched with other mixtures containing another color with a reddish component; as for instance,

360° carmine = 105° blue +255° red
360° reddish yellow = 65° white+267° black+28° red
360° violet = 58° white+225° black+77° red

(2) *Orange.* D had a particular aversion for this paper. It annoyed him because he could not characterize it adequately. Except in dim light it ordinarily had no appreciable color, but when he examined it closely he suspected red. *When his eyes were tired* he did not suspect color, and if, as he said, he was not "watching for color" *he would judge a black and white mixture to match it.* On one occasion he described it as looking "just like a broken steel surface"—"uneven"—"once heard it called a 'hard blue-grey'."

In ten such trials, in which he accepted a black and white mixture, the average proportions were black, 198.8°, and white, 161.2°, when the amount of white was decreased from 360°, and 220° of black and 140° of white when the black was decreased from 360°. The average of the twenty equations is

360° orange = 209° black+151° white.

The orange disk can also be matched with red, violet, or orange, and black or white. Notice this list:

360° orange = 265° reddish yellow + 95° black
= 285° violet + 75° black
= 100° blue +260° black
= 342° yellow + 18° white

The following matches were correct in shade, but not in color: notice the greater amounts of black needed in the last two as the greens approach the blue.

360° orange = 160° yellow-green +200° black
= 170° green +190° black
= 160° blue-green +200° black
= 140° green-blue +220° black

(3) *Reddish yellow.* Reddish yellow also appeared colored under the most favorable conditions. D says that "the color is not evenly mixed;" "it looks a little as though it might have been mixed red and white, and again as though it might have been black and white *unmixed.*"[15] At another time he said that he would infer color from the surface, for "it doesn't stay flat. If I have to make a guess at it, I'll call it a blue."

D insisted that reddish yellow and black would not mix to make grey, that "it mixes in black and white chunks, and doesn't make an even surface." He noticed a change in surface when 15° white, or when 38° black were added to the reddish yellow.

The following equations were satisfactory for "shade," but not for brightness *or texture*, and were made when he was not regarding the *color*, either on account of fatigue, or of rather strong illumination.

360° reddish yellow = 126° black + 234° white
= 105° black + 255° yellow
= 44° black + 316° yellow-green
= 65° black + 295° blue-green
= 112° black + 248° greenish blue

No match could be made with green. The contrast considerably accentuated the green of the inner disk, so that on one occasion, when he did not at first notice color in the green, he remarked when he saw it against the reddish yellow, *"That's* a good green, where did you get it?" Reddish yellow and black mixed almost invariably raised in D a suspicion that red had been added. The following mixtures contain the red that he demanded.

360° reddish yellow = 65° white + 267° black + 28° red
= 35° white + 25° black + 225° red + 75° yellow
= 49° white + 85° black + 117° red + 109° blue
118° reddish yellow + 242° black = 270° black + 90° violet

As a mixture of black and blue has a "reddish cast"

360° reddish yellow = 307° black + 53° blue

(4) *Yellow.* The yellow disk will exactly match a mixture of black and blue as to shade, but never as to brightness, as the yellow is duller even than a mixture of black with blue. A black and white mixture will exactly match a yellow, so well that D once remarked, "That's a well mixed pair of greys—we don't often get them better." An addition of a small amount of black to yellow gives a texture as of a colored surface; amounts upward of 15° of black have several times had this effect. D characterized this surface as brown; when asked if there could be color in it, he remarked that if there were it was so well mixed that it was "not evident *as color.*"

Such equations as these were obtained:

360° yellow = 310° white + 50° black
= 181° black + 179° blue
= 135° white + 225° blue-green
= 121° white + 239° yellow-green
= 28° black + 332° greenish blue

In no case did he attempt to match a mixture containing red with a yellow. As yellow is darker than blue, 181° of black were necessary to dull the brilliancy of the blue to the shade of the yellow.

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[15] On one occasion when D was being closely questioned regarding his statement of an *uneven* or *not well mixed color* he replied: "The color isn't smoothly mixed, it is on in chunks. You know how black and white granite looks when you look at it closely: can't you imagine how it looks when you are just too far away to see the *pieces* of black and of white, but not so far away that you see it as all one grey?—That is how these *unmixed* colors and greys look."

(5) *Yellow-green.* In naming the colors of the disks, which he did only upon request, D sometimes called yellow-green a dull blue. An added increment of black resulted in a change to a color texture, and in a certain amount gave a fairly good green.

46° yellow-green + 314° black = "fair green."

Yellow-green mixed with blue-green, even in illumination that cancelled color in each, resulted in green. The best proportions for green that D could get with this mixture were

138° yellow-green + 222° blue-green.

Any attempt to add red to the mixture on the inner disk was detected as soon as the mixture began, for the experimenter, to take on a red tinge, and sometimes before. Often the yellow-green induced a red in a neutral mixture.

The following are representative mixtures:

360° yellow-green = 260° yellow + 100° black
= 142° blue-green + 218° white
= 125° black + 235° white

In shade, but not in color,

360° yellow-green = 80° white + 280° reddish yellow
= 126° white + 234° blue-green
= 52° black + 308° green
= 40° black + 320° greenish blue
= 262° black + 98° blue
= 132° black + 228 yellow

(6) *Green.* The green of these papers was, according to D, too light in shade to be a good green. Often under ordinary illumination he did not notice the color. A slight decrease in illumination brought out the color very noticeably. The addition of black also brought out the green, when without the black no color was observed.

The best green for him was:

36° green + 324° black.

D often spoke of "a flash of green" or "flashes of green." These 'flashings' are characteristic of a surface on which the color was about at his green-limen. They are so brief that often he could not be sure what the color was, for he sometimes, though rarely, observed red in "flashes." *It was under these circumstances that he insisted that the state of his attention was a much more important factor than he felt the investigator seemed to believe. If he were expecting color he said that he watched for the flashes; if he was not watching for them he did not attend to them, therefore he did not see color in a surface that had color for him when his attention was keen.* He was very emphatic on this point, and his comments on these faint green surfaces seemed always to corroborate his emphasis.

On bright days

360° green = 164° white + 156° black
= 180° blue + 180° black
= 332° blue-green + 28° black
= 308° yellow-green + 52° black
= 198° yellow + 162° black

He did not attempt to match green in color to any reddish mixture. In the case of each of the group of greens there was a strong tendency to induce red and thereby render futile the effort to match these greens with a black and white mixture.

224° green + 136° red = 53° white + 307° black.

(7) *Blue-green.* These papers, D said, have the texture of color, and sometimes the colored flashes came as he first looked at the paper, and did not come again. In this connection he spoke of "colored flashes" but not of

"green flashes." When questioned, he said that he would suspect it of green on account of the 'shade' being lighter than a red surface.

360° blue-green = 172° black + 188° white
= 120° black + 150° white + 90° yellow
= 18° black + 108° white + 180° yellow + 54° blue
= 116° black + 174° white + 70° blue

The equation could be maintained by the addition of yellow, or blue, or both. In the last equation a red tinge was noticed, either a contrast from the blue-green background which he has obtained at other times or the effect of the mixture of black with blue which has been spoken of before.

Also:

360° blue-green = 185° black + 175° blue
= 105° black + 255° greenish blue
= 75° black + 285° green
= 84° black + 276° yellow-green
= 113° black + 247° yellow

Blue as usual required the most black to reduce it to the tone of blue-green.

Disregarding color, but matching in "shade"

360° blue-green = 68° white + 292° violet
= 105° white + 255° orange

The orange needed the more white and the violet the less to be reduced to the "shade" of blue-green.

(8) *Greenish blue.* This paper afforded a match for mixtures of black and white, blue-green and white, green and white, and yellow and white in the following proportions:

360° greenish blue = 314° white + 46° black
= 294° white + 66° blue-green
= 123° white + 237° green
= 83° white + 277° yellow

In "shade," but not in color, the following equations were good:

360° greenish blue = 90° white + 270° reddish yellow
= 130° white + 230° violet

(9) *Blue.* The standard blue "sparkled" in a way unlike any other disk of the series. This disk was *very* fatiguing, and D found work with it unpleasant on this account. The surface was smooth and white, in fact whiter than a somewhat soiled white disk, as was shown by the fact that, in 5 out of 10 trials, the white could not be made light enough to equal the "shade" of the blue. He once remarked, "How that centre white does shine; it is terribly hard on the eyes." Asked for synonyms for his use of "shine," he discussed the terms *sparkle* and *glint*, but here preferred "shine."

On this account few equations could be made that were even fairly satisfactory to D, and none that were entirely so. The blue, of course, could be mixed with any colorless disk to get the shade of the uncolored disk, but none could be mixed with white to match blue. For example:

360° reddish yellow = 18° blue + 342° black
360° yellow = 64° blue + 296° black
360° yellow-green = 260° blue + 100° black

As the mixture of black and blue had a reddish tinge, these matches were only in shade. His statement that such a mixture had red in it is borne out by the fact that it would induce green on a contiguous grey surface. Possibly this red was due to the decrease in illumination which may change the hue as well as the intensity of the surface, hue being a function of wave-length *and* intensity.

(10) *Violet.* D described the violet as "Looks like grey with some red in it. It is very light." He often referred to it as "the very light red." It could not be matched in color with black and white, for red must be added; also it induced green in a colorless surface, which added to the difficulty of obtaining color equations.

360° violet = 225° black + 58° white +77° red
= 36° black + 214° red +110° blue
= 206° white + 154° red
= 84° black + 276° blue
= 72° white + 288° reddish yellow

230° violet + 130° white = 360° purple
270° violet + 90° black = 185° red + 175° yellow
285° violet + 75° black = 360° reddish yellow

Violet, then, could be matched in color with any mixture in which a small amount of red was apparent, but could not be matched in color with any mixture in which green was apparent.

(11) *Purple.* D expressed difficulty in color matches for purple. It had a "tone" to it which necessitated the mixture of some neutral, and this of course was detected as a greyish tendency in the surface.

360° of purple could be matched in color by a mixture of white or black with red, orange, or violet; or by a mixture of black and blue which resulted in a reddish surface; and could be matched in "shade" by a mixture of white and yellow-green, yellow, or blue-green. D asserted that, in the case of "thin reds" and greens, he could disregard the slight amount of color and match the shade or brightness only.

The following is a representative series of matches for purple:

360° purple = 90° white + 270° orange
= 130° white + 230° violet
= 54° black + 306° blue
= 44° black + 12° white + 304° red
= 105° blue +255° red

326° purple + 16° black + 18° white = 101° blue + 259° red

The following three equations refer to matches in shade, but not in color.

360° purple = 18° white + 342° yellow
= 28° white + 332° yellow-green
= 50° white + 310° blue-green

Summary. These results show that red, purple, and violet can be matched in color with one another, *plus* black or white or both, but not in color with yellow, yellow-green, green, blue-green, greenish blue, or blue; that green, yellow-green, and blue-green can be matched in color with one another when mixed with black or white or both, but not in color with red, purple or violet; that yellow *plus* white can be matched in color with certain proportions of blue *plus* black, but not with any green or any red.

The ability to detect small amounts of red is apparent, and the lack of ability to observe small amounts of green under certain conditions of saturation and illumination is quite as apparent. Yellow is equivalent to light grey, and blue is equivalent to white; neither has color, and the blue is "brighter" than any white available.

After-Images

D had never observed positive after-images, and was not able to obtain them under experimental conditions. A light of low intensity did not seem to produce them, and the fixation of a stronger light was to him exceedingly unpleasant, and rendered his eyes incapable of further use for a considerable length of time. In the fixation of even a low c.p. incandescent lamp he did not see the glowing wire, but only a mass of light. After a few trials, the attempt was abandoned.

For obtaining negative after-images, disks of colored papers of the Hering series were used. These disks were 2 cm. in diameter, and were unmounted.

The first attempts were made in daylight in a north-exposure room. A white background was employed for both the fixation of the stimulus and the development of the after-image. The disks were given in irregular order. The following are typical results of such a test:

AFTER-IMAGES. TEST IN LIGHT ROOM

White background both for development and fixation. Diffuse north daylight, cloudy day; 10 a. m. D "feeling fine". ‡Stimuli, Hering disks taken in irregular order. The reds, violet, and purple are judged to be "colored."

STIMULUS	AFTER-IMAGE
Red (no. 2)	Barely bright spot
Red (no. 1)	Nearly white
Orange	*Very* light grey
Reddish yellow	White spot
Yellow	Medium grey
Yellow-green	Light grey with thin red
Green	Thin red
Blue-green	Thin red
Greenish blue	Light grey
Blue	Grey
Violet	Light grey
Purple	Very light grey

It is apparent that the only colored after-images he has perceived are those from the green stimuli, the color of which he has not perceived. His failure to obtain colored after-images from a red stimulus is obvious. Assuming that here, as was so pronounced in the spectrum tests, the strong illumination, accentuating the brightness of the stimulus, obliterated the color in the after-image, a change was made in the conditions of the test. A dark room was used,[16] the background for fixation and

[16]An attempt was made to obtain data on the Purkinje phenomenon, but this was abandoned. It was found, however, that D possessed very keen twilight vision, as he was able to discriminate small objects in the darkroom when the experimenter could not, with dark-adapted eyes, see a large sheet of white paper passed back and forth immediately before her eyes. D insisted that he had always been able to see his way about at night even when comrades had collided with objects in what they had termed "pitch darkness."

development being illuminated by single incandescent electric lights of different candle-power values. The comparison of these results shows mainly the effect of the decreased illumination of the stimulus, of the fixation background, and of the development background, although to what extent these differences are a result of partial dark-adaptation of the retinas is not known, but should be determined.

AFTER-IMAGES. TEST IN DARK ROOM

White backgrounds. 2 c.p. light in opaque metal reflector hung 9 inches above horizontal background, so as not to throw light into reagent's eyes. The red, green, violet, and purple under this illumination have color. D "feels fine."

STIMULUS	AFTER-IMAGE
Red	Dark grey
Orange	Same as the first
Reddish yellow	"Brilliant light grey"
Yellow	Shade darker
Yellow-green	"Nice pink one, different surface from any other"
Green	Very faint red
Blue-green	Light brown
Greenish blue	Very bright grey
Blue	Bright light grey
Violet	Light grey, dull
Purple	Medium light grey, fairly bright

Here as before he reported red after-images from green disks which he perceived as colorless, and reported *no* colored after-image for a red disk that represented to him almost a maximum of color. Yet the green disk, to him colorless under high illumination, was perceived as colored under low illumination. With the same illumination, but a grey background for both fixation and development, he suspected a tinge of green in the after-image from a red stimulus but "wouldn't swear to it."

With 4 c.p., and white backgrounds, the results were practically the same as above and so are not quoted, but with 4 c.p., and grey backgrounds, he developed for the first time a green after-image. The stimulus was the purple disk.

When a 2 c.p. was substituted for the 4 c.p. the result was the same except that the green was a lighter green. When the two reds were fixated simultaneously side by side and developed under the same conditions (grey backgrounds, 4 c.p. illumination) the hue of the green after-images was the same, but the after-image of the red (no. 2) was brighter and lighter than that of the red (no. 1).

With 16 c.p. illumination, and white backgrounds there was no color in the after-images of the reds, and only one green (the standard green) gave a colored after-image, whereas under the previous light conditions the yellow-green, the green, and the blue-green produced colored after-images.[17] The higher illum-

[17]Illuminations of 32 and 64 c.p. were objectionable to D and were, therefore, not used.

ination, it seemed, annulled color in the after-images. In this illumination either fixation or development on grey of a red stimulus gave only a "suspicion of presence of green on account of the texture; it gives the surface of green but not the color." The stimulus fixated on white and the after-image on grey revealed no color. Black backgrounds used with grey and with white interchangeably for fixation or for development produced no color results.

Even though a white background is most effective for rendering a low degree of green visible, it is not most effective as a fixation or development background for the production of green in the after-image. Nor is any use of a black or a white background so conducive as is a grey.

At the time of the latest test (1914) D perceived a very feeble green in the after-image of an orange stimulus fixated and developed on white under the ordinary indirect illumination of a dark cloudy day. On the same occasion and using the same stimulus when grey was used as the background, he remarked, "The darkest green ever in an after-image." He reported a "good green" on black, and none at all whenever the stimulus was fixated upon white, though it was only when a green stimulus was against white that its color was perceived.

The comparison of brightnesses in the after-images offers another opportunity for getting at the matter of relative brightness of different bands in the color series. For this purpose paired comparisons were made. Two disks of different colors were placed tangent to each other upon the white fixation sheet, and D was asked to compare the brightness of the after-image of the two. This was done in diffuse daylight.

AFTER-IMAGES COMPARED FOR BRIGHTNESS AND COLOR

Notes assembled from work in daylight on various days. Stimuli referred to by number in pair as indicated.

STIMULI		AFTER-IMAGES
Red (no. 1)	Red (no. 2)	About alike
Red (no. 1)	Orange	1 is darker
Red (no. 1)	Purple	2 darker
Orange	Reddish yellow	1 lighter, "stimulus of 1 was darker, though."
Reddish yellow	Yellow	Hard to judge; 1 *seems* darker
Yellow-green	Green	No difference in brightness, 2 redder, 1 lighter
Yellow-green	Blue-green	1 lighter
Green	Greenish blue	1 redder than 2, the 2 slightly lighter; close match
Green	Blue-green	1 redder, 2 thin red
Blue-green	Greenish blue	Alike in brightness, but 1 has red
Greenish blue	Blue	2 brighter
Blue	Yellow	2 lighter than 1, 1 dull
Blue	Reddish yellow	1 brighter but no lighter
Blue	Reddish yellow	Same shade but 2 has "glint"
Violet	Purple	No difference

Not all possible combinations were made, but those tried out are consistent with other remarks concerning the relative brightness of the series of papers and of colors in general.

The after-image of blue was duller than that of yellow (the stimulus blue, he said, was "bright," and yellow "dull"). The after-image of reddish yellow had the "glint" of a blue surface.

This test could have been carried farther very profitably, but time was of consequence.

Simultaneous Contrast

Only incidental observations were made regarding contrasts, but these were frequent and pronounced. In working with the color mixer, a green often induced a red on the contiguous grey surface, and this at times when the green was not perceived as green. Red more rarely induced a green, but this happened several times. This color induction often made it impossible to match a subliminal green with a black and white mixture, on account of the presence of the induced red.

D judged the series of Hering papers (excepting the purple) put up in squares of varying sizes—0.5 cm., 1 cm., and 2 cm.,—on a black, a light grey, and a white background at a distance of 18 inches from the eye. The reds and the violet he judged in each case to be red, and the green on white and on light grey he judged to be green, but the green on black was to him in every case colorless.

After-images in the Peripheral Retina

The possibility of peripheral after-images had not been mentioned to him. One day while the peripheral field was under investigation he remarked that these observations were difficult to make because at times the "spots" seemed to act so unaccountably—to come and go abruptly, with flashes of color and changing texture—changes so rapid that they were hard to keep track of. He asked if after-images could be obtained so far out. Receiving an equivocal answer he began speculating and observing, and decided that they were after-images, and that his previous reports had probably been "no good." On account of the lack of time, of apparatus, and of a well controlled method of experimentation, no attempt was made to investigate the conditions of these peripheral phenomena.

Indirect Vision

In December, 1910, an attempt was made to ascertain the facts regarding D's retinal field. A Hering campimeter was used, and a rough survey gave evidence of fields for both red and green, the green being much smaller. There was no point on the retina at which blue or yellow was judged as color. The

field for red extended irregularly from about 30° to 60° out from the fovea, and the green *very* irregularly from about 5° to 35° out, the short axis being on the left *retinal* meridian of each eye. The really interesting facts were the discovery of three color islands, two in the left and one in the right, farther out in the periphery than his so-called limits for color. These islands were small areas sensitive to red and to green *entirely surrounded* by an area insensitive to color.

A few readings with a Skeel Perimeter (given in the Skeel terminology) under dubious illumination and background control revealed the following facts (June, 1914).

D's left eye showed a normal field for white on the nasal retinal area, but a large blind area developed *within* the field as an *island*; it began on about the 150° axis, and extended from 32° to 50° out; on the 155°, it extended from 22° to 66°; on the 170°, from 20° out to 86°; on the 210° from 15° to 86°; on the 220° from 20° to 86°; on the 240° from 28° to 60°; and on the 270° from 35° to 42°. This blind spot extended roughly 70° in its widest portion along an axis, and from axes 150° to 270°.

His right eye showed a blind spot on the lower axis (at 270°) of about 10° in diameter. The sittings were discontinued before the entire retina could be thoroughly plotted.

Red, coming in from the periphery was judged to be white; going out, it "gets *thinner* in color just before it goes," and occasionally "goes out in a flash of white." It "darkens as it comes in." Green went out and came in as white. His field for red, left eye, showed blind spots practically coextensive with that of the white except that there seemed to be several widely scattered color islands within this large area, where islands for brightness had not been discovered. This must have been due to his greater attentiveness where he perceived the stimulus as colored, as he felt that he had to "watch colors closer." The field for green had the same blind spots and islands as the red. These should have been thoroughly mapped out, but D was in town for only a short time and we could not work long at a sitting.

Summary of Facts

It is evident, from these results, that this case of color-blindness presents unusual characteristics. The question of the name of the class to which it should be assigned is not nearly so important as the determination of the facts regarding the case.

In the test with the Holmgren wools the subject matched without hesitation the red skein with other skeins, all reds. The green skein he matched with greens, but with only a few of the available greens, and with skeins of no other color. He was slow in matching the blue skein and selected as matches many of the very light tints, the light blue-greens, greys, green-blues, and yellows. The yellow skein was matched with the same range of tints, but darker in shade. His replies to questions as to the basis upon which he made the matches were direct and consistent. "Red and green are colors and may be matched as colors; those two other skeins are uncolored and must be matched on the basis of brightness and texture." In the Nagel

test he picked out correctly the all-red-spots card as well as cards with some red spots; he also picked out correctly cards with some green spots but failed to find the card with only green spots. He could easily distinguish between the yellow-green and blue-green of the card A-6, but not on the basis of color. (According to the Nagel tests he would be classed as one of "die sehr seltenen Fälle von Blaugelbblindheit".) Work with the Hering color mixer (transmitted light) was futile; he found the four colored glasses to be red, green, grey (yellow) and white (blue). No matches could be made to satisfy him.

In color matching with Hering papers, he referred always to the red, violet, and purple papers as reds, though the experimenter was always exceedingly careful to use no color names. He did not often refer to the "green" of the paper, but when he did, he used the term correctly. He usually adopted the experimenter's terminology of "this" or "that" in referring to other papers, though he occasionally spoke of them as uncolored.

In the examinations with different spectra he judged both the red and the violet ends to be red, even distinguishing the red from the violet end by referring to it as the 'good red' as contrasted with the 'thin red' end. He was insistent that any band in the thin red could be matched with some band in the good red end in color though not in brightness, as the thin red possessed a brilliance peculiar to itself. When he referred to color in the green band, he was consistently correct in both his color judgment and his color naming. The blue region he referred to always as white, and the yellow band sometimes as white, though usually as having no color.

A careful examination of his color sensitivity made with a Rowland 6-inch grating of a 21 foot radius with sunlight, gave the following limits for the different bands: red, 7620 A — 6050 A; neutral, 6050 A — 5600 A; green, 5600 A — 4920 A; neutral, 4920 A — 4575 A; red at violet end 4575 A — 3933 A. The brightest point in the spectrum with the carbon arc was about 4300 A. It was white and most brilliant. This point is in the blue. With the arc light spectrum, he saw as red the lines at 3850 A, which for the ordinary eye are not visible, and his description of the region was correct. In the potassium spectrum he saw as red the lines at 7699 A. Other observations support these determinations.

The spectrum diagram indicating the colored and neutral bands is based on these readings (v.p. 165) as these were most painstakingly obtained with the best conditions and apparatus. These readings are therefore the basis upon which his spectrum color judgments should be discussed, except as indicated on p. 156.

The green band could be observed only by cutting down the intensity of the source of light.[18] With sunlight spectra he located the brightest point twice at 4650 A and once at 4680 A. (These are in the blue.) The best green was judged with a prismatic spectrum once with a diffuse light at 5090 A and once with sunlight at 5010 A. (Professor Gale thinks the best green, for himself, is at the magnesium lines, C and C_2, at 5184 A and 5174 A respectively.) Often, when near the limits of the green bands, D complained of an alternation of green and red at the same point. The possibility of after-images had not then occurred to him, and he was disturbed.

When working out color equations with Hering papers it was found that the reds, violet, and purple can be used with black or white or both in matches in color with one another, but not with yellow, yellow-green, green, blue-green, greenish blue, or blue. Green, blue-green and yellow-green can be matched in color with one another when mixed with black or white or both, but not with the reds, violet or purple. Yellow mixed with white can be matched in color with a mixture of blue and black, but not with any green or any red. He is sensitive to small amounts of red in a mixture, but not to small amounts of green, except when mixed with black. The question as to whether D sees red and green as the presumably 'normal' eye sees them cannot, of course, be determined. But it is no more of an issue here than in comparing the color vision of any two people.

After-images were colored, uncolored, or grey. Those following the fixation of a green were red or uncolored, depending upon the brightness conditions. They were never called red when they did not follow the fixation of a green, though often the green in the stimulus was subliminal. Under low illumination he perceived green in an after-image following a fixation of

[18]The narrowing of the green band is dependent upon the intensity of the beam and is not a condition necessarily peculiar to a colorblind eye. Professor Gale tells me that the discrepancies in the width of the yellow band shown in colored spectrum plates in various textbooks is apparently due to the intensity of the beam used as a source of the spectrum from which the copy is made. With a beam of sunlight direct upon the slit, the yellow band is *very* broad, reaching even into the blue. With a narrowed slit and indirect light the yellow band becomes narrow. Considering D's sensitiveness to brightness and its effect of obliterating color, it might easily be that his apparently diminished sensitivity to green is instead sensitivity to brightness, much in excess of that of the ordinary eye.

a purple or a red disk. After-images resulting from the fixation of a reddish yellow disk had no color but unusual brilliance (that of a blue). Those from other colors were different tones of grey.

When a green mixture on the color-wheel was against a grey background, red was often induced in the grey. This happened both when the green was perceived and when it was subliminal. His insistence that the mixture of black and blue contained a small amount of red is verified by the fact that it induced green on a contiguous grey. In working with a prism spectroscope, he often commented on the fact that the slit was green, with red on the edges of the aperture. On one occasion, when observing a "good green" band with a sunlight spectrum he remarked, "The sun went under and left the disk red. Why was that?"

The Frauenhofer lines, however, were often spoken of as colored, against a colorless background, when the intensity of the band obliterated the color. For instance, with a green band of strong intensity, he would remark: "There is no color in the slit, but the lines are green." In nearly all the observations the color was noted at the lines before being noted in the background.

The extent of his peripheral visual field, though not very thoroughly examined, showed a much more restricted area for green than for red. Yellow and blue were judged always as grey and white and therefore there were for these, as colors, no fields. He frequently obtained red after-images from green stimuli in the indirect field in regions in which he was insensitive to green. These after-images were strong enough to obliterate the color of the stimulus, and the rapid fluctuation of colors caused him both confusion and annoyance. Several scotomata were found.

There were several interesting 'color islands' in his peripheral field, that is, small regions sensitive to red, and affording green after-images for red, and sensitive to green, which were entirely surrounded by areas insensitive to color.

D had developed several effective secondary criteria in judging colors. These were texture, flatness or smoothness of surface, shade, brilliance or dullness, and mixture. Colored or uncolored surfaces had a texture that grey surfaces lacked. (This texture had nothing to do with textiles, as paints had it.) Uncolored surfaces were not *well-mixed* and *flat* as were grey surfaces. Orange was a particularly "unmixed" surface. A mixture containing red was darker in *shade* than green when both were subliminal, and this shade was sometimes the basis for distinguishing green. Blue was *brilliant* in surface—had a *glint* to it—while yellow was *dull*. Purple and violet were both

described as being "thin reds" with the *lustre* that characterizes some blue surfaces. With the help of these secondary criteria he concealed his color-blindness from his friends and even from his students when he was teaching physics in a small High School. The experimenter had as much difficulty in understanding what these criteria meant to him as he had in understanding what blue and yellow were, as colors, to her.

The family history shows color-blindness on both sides, and the father and mother are distantly related. There is every reason to believe the case to be one of congenital color-blindness. D believes that his color discrimination is undergoing a deterioration, though he has chosen colors in the Hering series in 1914, 1916, and 1921 as he did in 1910. He says also that he cannot now (1921) use his eyes for study for so long a period as he did in his student days, and he is sure that the acuity of the left eye, particularly, is diminishing. There are, then, apparently degenerative phases of the case. But it is probable that the categories, degenerative and congenital, are not so distinct as in our literature, at least, they are assumed to be.

Conclusions

This case of color-blindness presents the following features:

1. No shortening of spectrum.
2. Sensitivity to red.
   Weak sensitivity to green.
   No sensitivity to yellow or blue *as color*.
3. Brightest band in spectrum in the blue.
   Blue-green next in brightness.
4. Brightness obliterates color as shown (1) in spectrum tests, and (2) in after-images.
5. Brightness discrimination keen.
6. Achromatic zone of vision normal in size.
   Red zone slightly constricted.
   Green zone much constricted.
   Both brightness and color-blind areas with 'islands' in peripheral field. Several scotomata.
7. Effective secondary criteria in judging color.
8. Congenital basis and possibly degeneration.

SOME PROBLEMS OF MEANING

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The purpose of this paper is to discuss the following problems of meaning from both the theoretical and experimental points of view. (1) Are the contents of meaning exclusively sensory and imaginal? (2) Is meaning structure, function, or both? (3) What is the relation between meaning and kinaesthesis? (4) What are the attributes of meaning? (5) What is the relation between meaning and the so-called mental elements? (6) What are the criteria of meaning? (7) A theory of meaning.

The first problem need not detain us long; for experimental literature overwhelmingly supports the view that meanings are exclusively sensory and imaginal in their content.[1] The author recently acted as subject in a prolonged introspective study of meaning,[2] typical introspections from which furnish added indications that such apparently indescribable processes as Kakise's "feelings of meaning," imageless thought, Moore's "pure meaning," etc., are kinaestheses which the reagent has failed to analyse.

[1]Pyle, W. H., An experimental study of expectation, this Journal, 20, 1909, 530-569; Okabe, T., An experimental study of belief, *ibid.*, 21, 1910, 563-596; Book, W. F., On the genesis and development of conscious attitudes, *Psychol. Rev.*, 17, 1910, 381-398; Jacobson, E., On meaning and understanding, this Journal, 22, 1911, 553-577; Kakise, H., A preliminary experimental study of the conscious concomitants of understanding, *ibid.*, 22, 1911, 14-64; Chapin, M. W. and Washburn, M. F., A study of the images representing the concept "meaning," *ibid.*, 23, 1912, 109-114; Woods, E. L., An experimental analysis of the process of recognizing, *ibid.*, 26, 1915. 313-387; Geissler, L. R., Analysis of consciousness under negative instructions, *ibid.*, 23, 1912, 183-213; Clarke, H. M., Conscious attitudes, *ibid.*, 22, 1911, 214-249; Fisher, S. C., The process of generalizing abstraction and its product, the concept, *Psychol. Monog.*, 21, 1916, 213 pp.; Fernberger, S. W., An introspective analysis of the process of comparing, *Psychol. Monog.*, 26, 1919, 161 pp.; Gleason, J. M., An experimental study of feelings of relation, this Journal, 30, 1919, 1-26; Wheeler, R. H., An experimental investigation of the process of choosing, *Uni. of Ore. Publs.*, No. 2, 1920, 59 pp.; Crosland, H. R., A qualitative analysis of the process of forgetting, *Psychol. Monog.*, 29, 1921, 159 pp.

[2]This Journal, 33, 1922, 223-233. Compare the introspection on the meaning "knife," p. 224, with Moore on a similar meaning, *Psychol. Monog.*, 27, 1919, 162.

See also *Psychol. Rev.*, 29, 1922, p. 425. Here it was found that when the reagent assumes an unanalytical attitude toward his experiences, meanings appear as complexes of content whose sensory and imaginal ingredients do not rise to the focus of attention; but in the presence of an analytical attitude these same meanings invariably stand out in their detail of exclusive sensory and imaginal content.

The results of these investigations demonstrate that "sensationalism" so-called is able to meet the issue fairly and squarely and to show a real identity between the content of "thought" and the structural elements which it recognizes.[3] "Thought" recognizes its structural elements in such fashion as is illustrated by "interpretative periods" in the act of choosing and in the development of meaning.[4] A recent effort to demonstrate the contrary can hardly be accepted as valid evidence.[5]

An extended series of experiments on synaesthesia with check data clearly supplements the evidence so far brought forward against imageless and sensationless meanings.[6] It seems a waste of time, then, to pursue our illustrations farther.

Our second problem deals with meaning as structure *versus* meaning as function. Jacobson[7] found that meanings of words varied as did their contextual associates, and that where such associations were absent so were the meanings. In our studies on synaesthesia this fact was clearly evident. Our synaesthetic reagent perceived and understood as did our asynaesthetic

[3]Demanded by Moore, *op. cit.*, 224.

[4]Wheeler, *Uni. of Ore. Publs.*, 1, no. 2, 33-35, 51. This JOURNAL, 33, 1922, 231f.

[5]Moore, *op. cit.*, and McDonough, A. R., The development of meaning, *Psychol. Monog.*, 27, 1919, 443-515.

The following is an introspection from one of McDonough's observers. "I do not know how to analyse this consciousness of knowing but I know the meaning. It is something of the same order as the recognition of the word as familiar. There seemed to be a judgment in both cases . . . . although I was not at the time conscious of judging" (477). All of Moore's and McDonough's introspections are of this type. Processes of knowing, recognizing, judging, realizing, waiting, imagining, defining and the like are mentioned over and over again with such dearth of analysis and vagueness of expression that we are forced to doubt very seriously whether their reagents ever knew what they were experiencing when they reported meanings.

If higher standards of introspective technique than these are not used in favor of imageless thinking and meaning, why waste time with experimentation at all? Indeed the psychologist need not go to the trouble of employing supposedly trained introspectors in order to find ample alleged evidence for pure meanings. Any untrained but intelligent person can tell us that he thought so and so, realized this, judged that, etc. Would the trained histologist conclude that his own observations were histological dreams because his untrained friend failed to see all that he himself could see? Cf. Moore, *op. cit.*, 223.

[6]Wheeler, R. H., The synaesthesia of a blind subject, *Uni. of Ore. Publs.*, 1, no. 5, 1920, 61 pp.; Wheeler, R. H. and Cutsforth, T. D., The synaesthesia of a blind subject with comparative data from an asynaesthetic blind subject, *Uni. of Ore. Publs.*, 1, no. 10, 1922, 104 pp.; Wheeler, R. H. and Cutsforth, T. D., The rôle of synaesthesia in learning, *Jour. Exper. Psychol.*, 4, 1921, 448-468; Wheeler, R. H. and Cutsforth, T. D., Synaesthesia a form of perception, *Psychol. Rev.*, 1922, 29, 212-220; Wheeler, R. H. and Cutsforth, T. D., Synaesthesia and meaning, this JOURNAL, 33, 1922, 361-384.

[7]*Op. cit.*, 564, 565.

reagent, but his mental contents were radically different. The content of meaning-contexts varied in our two reagents, but the function involved, *i.e.*, the behavior of these contents, was identical. Further, where in our synaesthetic reagent the behavior of contents within different contexts varied, meanings varied.

Clark[8] pointed out that functional rather than structural stages differentiated familiar, generic and unfamiliar images. Meanings, then, were not demonstrable in structure and became meanings only when they behaved in certain fashion with respect to associated imagery or sensory processes.

Our own findings are in direct accord with Clark's and Jacobson's, in that no mental process turned out to be meaningful in the absence of context and shifting contents. In our synaesthetic reagent meanings developed by means of shifts, enlargements and complications of colors, together with developing synaesthetic visual imagery of kinaesthetic sensation. Unless mental contents of sensation and image developed from antecedent contents before the latter had entirely vanished from consciousness, meaning invariably failed to develop. Everywhere a detached or dissociated process meant nothing. Conversely, a given mental content contributed to meaning only when it led to a subsequent content or to a motor response. In fact a motor response seemed quite as essential to the development of meaning as did imagery and sensation in context. After all, this is what one would expect if meaning accrues to the behavior rather than to the content-aspect of experience; for of what value in mental life is the process-aspect[9] of experience unless it should be motor in character, leading to a motor adjustment of the organism?

In our view, then, meaning, as structure, is sensation and image or it is context; as function, it is the behavior of sensation and image or it is motor response. No treatment of meaning is complete if confined either to structure or to function alone. Structures imply concomitant functions and *vice versa*. In evidence of this we need only point to Moore's statements in spite of the fact that he insists meanings are structures only. After defining meaning as a structure[10] he writes, "all meanings as conscious events" etc.[11] He admits after all that meanings are functions; for an event is something which takes place; it is action of some content, some structure or some object. We read further that "meanings have the power of calling forth other meanings."[12] Need it be pointed out that meaning, structurally considered, does not call forth other meanings;

[8] Visual Imagery and Attention, this JOURNAL, 27, 1916, 466, 491.
[9] Cf. Crosland, *op. cit.*, 118, 119, 133.
[10] *Op. cit.*, 185.
[11] *Op. cit.*, 189.
[12] *Op. cit.*, 190.

that something "static" never did anything? A structure is by definition inert, inactive and arbitrarily separated from everything else. Obviously then it is meaning as function or action which calls forth other meanings.

Again we read: "meanings are capable of being in the focus point of consciousness." [12] But as structures how did they get there? Before a content can be conceived as entering the focus of consciousness it must be regarded as changing or moving in some way; it arrives in focal consciousness either by having been placed there bodily by some external agent or else it "flows" or "processes" in. In the latter case meaning, of course, is dynamic or functional as well as being static or structural.

We have said that meaning is both structure and function. A corollary of this view is found in the evident fact that what is function on a given level of complexity of phenomena becomes structure on a higher level of complexity. For example, the contents of a task-consciousness, to choose, may be exclusively visual imagery, vocal-motor imagery and bodily kinaestheses. With respect to itself such a task-consciousness is not content; rather it is function; for it consists in the fashion by which sensations and images operate in the individual's behavior. The rôles of such processes, the work they perform, make the complex a task-consciousness. Further, the task-consciousness cannot be construed as content in this situation because, upon examination, the content is seen to vary from individual to individual and in the same individual from time to time. The content is not uniform while the function is uniform. On the other hand, with respect to something more complex, namely the entire act of choosing, the task-consciousness, now definable as a phenomenon of meaning, becomes a content. It is a content of the volitional consciousness. Likewise volition, as such, is not content but function; but with respect to a prolonged and more complex period of mental life it is content, along with reasoning, judging, etc.

The same consideration holds for the *Bewusstheit*, the *Bewusstseinslage*, the feeling of relation, the feeling of activity, consciousness of self,—any meaning. Abstracted from its setting along with equally complex processes, no one of these phenomena can be regarded as a structure. They are all functions or modes of behavior whose structures are exclusively sensation and image. Regarded less analytically, however, they become unanalyzed structures when referred to a more complex mental process of which they are the parts.

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[12] *Op. cit.*, 190.

A second corollary also follows from the view that meanings are both structure and function. Meaning and image or sensation are equally important in consciousness. We are hardly falling into a logical dilemma, as Pillsbury thought,[14] by positing two ultimates in consciousness, meaning and image (or sensation) with no possible interrelation between the two. When meaning and image exist together, the latter is the content of the former; the behaving image is meaning. Evidently structure and function are two ultimates; they are mutually exclusive but dependent factors, by definition opposite, thus defining one another. An essential relation does exist between these two factors in that one is inconceivable and has no meaning without the other; one implies the other. Thus, rather than falling into a theoretical dilemma, we are obeying an inevitable law of logic, namely, that nothing functional is conceivable without its structural aspect and *vice versa*. Function, let us say, is the thing itself and structure is the "form" in which the thing exists. In other words, structure is the mode of taking place of function; it is means, while function is the "end." [15]

Our third problem deals with the relation of meaning to kinaesthesis. One need not point to the numerous attempts in psychological literature to explain a problem too simply by reducing a great many varied phenomena to a common element. Nevertheless, the relation of kinaesthetic processes to meaning has received belated recognition. We are adjusted continuously to one stimulus or another; incipient strains and delicate movements are always present; in fact kinaesthesis is so common in mental life, its localization so wide-spread, its complexes and variations so numerous, and its changes so delicate, that an introspective description of almost any process whatever is incomplete until incipient changes in tonus and delicate movements are taken into account. Until it is generally observed that kinaesthesis is always with us and that it functions in all mental activities, we are violating a scientific law dealing with the control of all processes associated with the object of experimentation. Indeed, ignoring kinaesthesis in a majority of introspective studies leaves it open to question whether the very essence of the desired process is overlooked.

In our view kinaesthesis is an essential component of the consciousness of meaning and the mechanism of the develop-

[14]Pillsbury, W. B., Meaning and image, *Psychol. Rev.*, 15, 1908, 150-158.

[15]Cf. Bode, B., *Psychol. Rev.*, 15, 1908, 255-264 and Woodbridge, F., *ibid.*, 15, 1908, 397-398, on the problem of meaning. No doubt their difficulty disappears when the concepts of structure and function are adequately applied to the problem of meaning.

ment of meaning is motor response.[16] That quality of experience which marks a given mental process as meaningful is unquestionably the quality of kinaesthetic sensation. Consciousness of meaning is a species of recognizing; it may involve either an incipient or a well-developed feeling of familiarity; attitudes of acceptance are frequent constituents of consciousness of meaning. Indeed no matter what the variety of that consciousness may be, its structural core is kinaesthetic sensation or image and its development—its essentially flowing character—depends upon the circumstance that the individual is responding to a stimulus motor-fashion. The stimulus is an antecedent complex of sensation and image. Meaning, then, is response to one's mental processes. A physiological stimulus causes a mental process, sensation and image; the latter is then response; the mental process then becomes a stimulus for further response; and the latter is meaning.

One need not look for a structural differentia of meanings in different patterns of kinaesthetic sensation, for he will find, no doubt, that kinaesthesis, as such, will not distinguish one meaning from another. Meanings are distinguished, labelled and defined by their setting and not by their content. For example, a visual image of a knife may be meaningful in virtue of kinaesthetic accompaniments but the meaning, knife, is not a datum of consciousness until the image is explicitly and concretely labelled either vocal-motor fashion or by such a process as visualizing a knife in use.

On the other hand almost inconceivably slight shiftings in muscular tonus about the face, or in the throat, constitute changes from the consciousness of one meaning to that of another, given corresponding changes in the mental processes to which the motor reactions are responses. When mental processes are syncopated, such delicate changes constitute the development of new meanings.

The fourth problem dealt with the attributes of meaning. Attributes of mental processes in general are divisible for practical purposes into two groups according to the structural and functional points of view. Let us confine structural attributes to the field of content or quality. These we will call substantive attributes. Color, brightness, tone, olfactory and gustatory qualities and the like would fall into this group. Such qualities possess certain attributive aspects—certainly not attributes on a par with quality—such as extent and possibly duration. On

[16] In the view of the writer such concepts as determining tendency, perseverative tendency, and like attempts to envisage the dynamic would be far more serviceable to psychology were they expressed in the behavioristic terminology of stimulus and response. Functional psychology is superficial and relatively meaningless until interpreted in terms of reflex mechanisms and motor reactions.

the other hand, mental processes possess functional attributes or attributes of behavior, a better name for which might be *modal* attributes for the reason that these attributes are really modes of behavior. In this group fall such factors as generality and concreteness of meaning, and no doubt clearness and complexity of mental processes.

Moore makes generality an attribute of structure. Further he insists that only pure meanings possess this attribute; that sensation and image do not.[17] We point out again that Moore's introspections were so faulty, so lacking in analytical detail which many other investigations unquestionably demonstrate, that they can hardly be accepted as serious competitors of Fisher's work.[18] Fisher, it will be remembered, found that generality consisted of the fashion-in-which contents behaved in consciousness. That is, generality was a mode of behavior of sensory and imaginal processes, describable in terms of function but not in terms of structure. In other words, generality turned out to be a modal rather than a substantive attribute. Obviously the same thing can be said of concreteness of meaning.

A second attribute of meaning is said, by Moore, to be "analysableness into simpler meanings." [19] All that this means, of course, is that meanings are complex, something which we have known for a long time. Moreover, could anything be more of a tautology than to assert that meanings are analysable into simpler meanings? Would a large dose of medicine be analysable into smaller doses if it were not medicine first and last? All analysis is the act of resolving complex meanings into simpler meanings. One does not analyse anything in mental life without analysing meanings. It is only meaningless experience which cannot be analysed. Moore seems to have discovered a very obvious fact.

We can hardly accept Moore's statement that "consciousness of relation" is an attribute of meaning.[20] No better place in which to observe consciousness of relation could be found than in acts of comparing. Accordingly, if we turn to Fernberger's[21] study of judgment, we note that consciousness of relation consists exclusively of sensory and imaginal contents. The "reference" to something which a relational consciousness always implies is a momentary shift of attention to some sensory or imaginal datum appearing at first in the fringe of consciousness and possibly becoming focal. In other words, the relational consciousness consists of more than one mental content; a

[17] *Op. cit.*, 198, 201.
[18] *Op. cit.*
[19] *Op. cit.*, 191.
[20] *Op. cit.*, 191.
[21] *Op. cit.*

change in intensity, quality or in some other attribute of sensation and image takes place; the reagent responds to this change by employing shifts, visual, verbal or other imagery. The meaning of the relation is not explicit until interpreted by a subsequent mental state. Moore's third attribute of meaning, then, accrues to behaving sensory and imaginal contents and not to attributes of structure.

Another attribute of meaning, according to Moore, is interpretativeness.[22] One is involved in function no matter how he defines interpretativeness; for if by it is meant a capacity or a potentiality, one refers to a latent function; and if by interpretativeness is meant that meanings interpret, action is again implied by definition. "Images do not interpret," Moore insists,[23] "they remain what they are . . . . images and not meanings." What then is the function of imagery in mental life? We let Moore answer this question; for he goes on to assert that images are illustrative of meaning after the meaning has taken place.[24] Surely, one illustrates a point for the purpose of clarifying the meaning; illustrations are concrete examples of the meaning. Why insist that an illustration is something beyond or aside from the meaning which it illustrates, when it is really a part of that meaning? Contrary to Moore's assertion, then, the very function of imagery is that of interpretation.

We now turn to the fifth point, the relation of meaning to the problem of the mental element; and we posit the following considerations. (1) There are no irreducible and *sui generis* mental contents, not excepting sensation and image as data of consciousness; (2) the simplest possible mental content derives its existence from the functioning of a companion-process. In other words, the simplest existing mental content is an analysable meaning, a dual and a synaesthetic process. (3) Unanalysability of structure only means that we have reached a point of introspective analysis beyond which no further contents can be segregated by attention. The simplest possible act of attending takes place in the presence of two contents, one from which attention may shift and one toward which it may shift. Further, added effort of attention results in a necessary shift to processes other than the object of analysis.

Our reasons for positing these considerations are as follows. First, in synaesthesia the primary sensation exists only in connection with a stereotyped associated image. The simplest possible existing mental content in the consciousness of a synaesthetic reagent is a dual process involving a shift of attention from a sensory to an imaginal component. In the absence of the

[22] *Op. cit.*, 191.
[23] *Op. cit.*, 216.
[24] *Op. cit.*, 216.

image-component the reagent has no means of knowing that the sensory component exists in consciousness other than in the form of an unrecognizable "plus" or "something." From a functional standpoint, however, synaesthesia is a normal process; it is an act either of perceiving or of cognizing. One half of the synaesthetic process defines or attributes meaning to the other half, making it possible for either one to become a datum of consciousness.

Secondly, when the synaesthetic reagent is instructed to attend solely to the sensory component and to ignore the imaginal component one of two things invariably happens: either he finds that he is unable to fulfill the instructions, or his attention becomes absorbed in strains of effort and the sensory component disappears altogether. In either case the result is negative as far as isolation of the primary process is concerned.

Thirdly, there are certain individuals—and the writer is one —who find that in their own consciousness sensation and image never stand alone. The writer has never, to his knowledge, been able to find a pure red (not excepting spectral lights) having in it neither observable yellow or blue; green always has either yellow or blue in it; blue has in it either red or green; yellow is likewise never pure, for it always has in it some observable red or green as well as white; every white has some black, and every black has some white, and so on. Similarly, cutaneous sensations are never "pure"; the writer never experiences a pressure sensation, for example, in the absence of a certain amount of visual imagery and kinaesthesis. No amount of practice entirely eliminates them. There is always a visual or a verbal tendency associated with organic and kinaesthetic sensation. Likewise olfactory and gustatory sensations are equally as complex. The important fact about all of this seems to be that any so-called "sensation" is inconceivable, unimaginable, and unrecognizable in the absence of the so-called secondary or companion-process. In connection with one quality there invariably appears another and different quality by means of which the first is perceived and by means of which the former becomes a datum of consciousness, a conscious content. Any so-called "simple" of experience turns out to be a complex.

Fourthly, the "flowing" nature, the process-aspect, of consciousness precludes the possibility of irreducible and *sui generis* processes of any kind. One experience, no matter how simple, is continuous with some other process, and the latter is at the same time alike and different in some way from the former; else the change or continuum would be inconceivable on the one hand and unrecognizable on the other. Thus the changing or durative aspect of a content is unobservable until there are two contents in consciousness, constituting a dual process and at the same time a "mental element."

Fifthly, before one can become aware of a simple content, a separate content is essential to provide the two terms of the subject-object (stimulus-response) relation. This additional consciousness is not consciousness as an entity, but it is another content, different in quality from the first, as a result of whose presence there follows an implicit process of judging. On the other hand, unless these two contents constituted the same, original and practically simultaneous experience, there would be no way in which a third act of consciousness (an interpretation) could identify either part of the original experience. All of this means, of course, that mental processes are not "given" or "existential." Indeed, if given, to what are they given? A better concept, by far, seems to be that the simplest datum of consciousness is not a "state" or a "content" which is "given" but is a function, a meaning, whose ingredients are observable only when they exist in pairs or in larger complexes. Under these circumstances the members of the complex derive their qualities from one another and from a subsequent interpretative period. The so-called "given" comes into existence by growth, by function; it is action involving a shift of attention from something to something; experience, then, is not given but creative; not passive but active.

Sixthly, if the simplest possible experience consisted of but one content, quality or attribute, by definition we have made that experience absolute; we have compartmentalized it and have precluded the possibility of ever becoming conscious of it. We have broken the continuum.

Seventhly, the position occupied by psychology in the sciences suggests that the psychologist's element is a complex rather than a simple. The element or basic structure of mathematics is the simple, absolute and fixed number, 1. By "1" is not meant any number smaller or larger than "1" no matter how slight. The unit is fixed and thus the science is exact. In physics, structures are more complex and the units or elements are less fixed as the facts of relativity show. By the time we reach chemistry more complex elements are added to the rapidly growing list of "simples" (units of time, space, vibrations, ions, etc.); and from the chemical elements are derived by synthesis a vast array of complex structures or substances running into unknown complexity in the organic compounds like the proteins. Hence from a structural standpoint chemistry not only becomes more complex than the sciences upon which it is based but this complexity is increasing in geometrical ratio.

The simplest strictly biological element, the cell, represents a vastly higher order of complexity of structure; and yet the increasing complexity advances, still, with accelerated speed, as these cells are found to be combined into anatomical systems

and tissues whose functions constitute the subject-matter of physiology and hygiene. Obviously, the variability of biological phenomena is derived primarily from the complex and variable nature of the basic biological unit, the cell.

Should we not expect, then, upon reaching psychology, that our strictly psychological unit or element—the sensation or image—and the element of behavior—the reaction—should be even more complex in proportion? New syntheses have evidently been built upon the structural and functional phenomena of biology. These syntheses we call consciousness and behavior.

We find that the biological, the chemical and perhaps the physical "elements" are analysable, although less and less so the nearer mathematics one approaches. Should we not expect to find our psychological element analysable even by our own methods of introspection, especially since complexity has been increasing with accelerated speed through the hierarchy of sciences? Obviously, the element of psychology is an analysable complex, a variable thing, whose natural variability and complexity are responsible for the difficulty of predictability in the science and for the so-called lack of exactness. Exactness will not rise above its source! With a variable and inherently "inexact" unit to begin with, added exactness cannot be found.

Seventhly, the nature of analysis itself would lead us to conclude that we should find nothing irreducible in consciousness. For example, what do we mean by unanalysability except to portray the idea that as yet no technique has been found sufficiently elaborate or delicate to tear apart a given phenomenon? Thus an element means nothing more than something which resists analysis; there is no excuse for calling it an irreducible or *sui generis* thing. "Element" should never mean something "ultimate".

So, in psychology, we regard a process as irreducible when it cannot be "torn apart" by means of attention-behavior. But when we have thus reached the limit, we fail to take into account what is happening to the act of attending, which, if the content had been reducible, would have resulted in the separation of the complex into its ingredients. Attention is not static and cannot be directed to an "irreducible" and there remain fixed! As in case of the sensory component of a synaesthetic complex, an "irreducible" is always a "step behind" attention. Evidently what happens is this: when we have reached a process so simple that under the circumstances it cannot be separated into further ingredients, the effort of analysis continues in the form of further shifts; but not being able to shift over a "simple" which has no

details, it must shift elsewhere. Hence the companion-process; and hence the so-called element as an analysable complex. [25]

Eighthly, the so-called attributes of sensation, namely, duration, intensity, extensity and clearness, do not seem to be irreducible features of experience. The trained synaesthetic reagent finds that the sensory component of the synaesthetic complex possesses no attributes. Its intensity, duration, extensity and clearness are data of consciousness only as meanings accruing to the associated image and its behavior. Extension, in the writer's experience, is in part actual or imaged movement; clearness is not an attribute of sensation or image but a matter of complexity of qualitative detail (cognitive clearness) or a matter of visualizing some process in focal or blurred vision (attributive clearness); duration is in part muscular strain and is never a simple attribute.[26]

If such a position is true it means that many investigations of sensation, perception and affection have led us to a misinterpretation of the facts or have failed to give us a true picture of the situation.[27] The reason for this may not be hard to find. Reagents are trained to ignore the so-called "secondary" processes in experiments on sensation and perception. In such a training, which requires several months in some instances, the intention is to teach the reagent to detect minor differences in sensory attributes; in actuality, however, there is evidently built up a mental "set" or bias toward meaning. As a result of the training he receives the reagent becomes less rather than more analytical toward the object of his introspection; he is prevented from noting the real complexity of the very experience which he is reporting (by the verbal-report method) as simple. A more complex introspection throughout such an investigation would guard against such a dilemma.

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[25]This situation explains why the synaesthetic subject thinks that in "seeing" a tone he is "hearing" it. The experience is a complex; it is context; it means a "tone" even in the absence of an outstanding auditory quality. It also explains why the expert introspector regards his experience of red, pressure, or what not, as a "simple." He is looking for a simple. The simplest datum which will stand alone in consciousness is "meaning", a complex; hence the meaning, pressure or red, is the experience which he designates as ultimate. In case either of the red or of the pressure it is more probably true that the experience is synaesthetic and a process which will stand out in consciousness as a complex the instant the bias toward meaning is eliminated.

[26]Cf. Calkins, M. W., Attributes of sensation, *Psychol. Rev.*, 6, 1899, 506-514; Rahn, C., The relation of sensation to other categories in contemporary psychology, *Psychol. Monog.*, 16, 1913, 131 pp.; Talbot, E. B., The doctrine of conscious elements, *Philos. Rev.*, 4, 1895, 154 ff.; Washburn, M.F.,Psychological analysis in system making, *Philos. Rev.*, 11, 1902, 445-462.

[27]Cf. Young, P. T., An experimental study of mixed feelings, this JOURNAL, 29, 1918, 237-271.

Let us take the case of pressure qualities. The purpose of a certain investigation may be to ascertain the number of distinguishable pressure qualities. At the outset the reagent may have reported visual imagery and incipient kinaestheses, but these are thought to be extraneous; under the preconception that pressure qualities are existential and not developmental he is instructed to ignore these secondary processes. Such reagents who ultimately fail to do so are regarded as possessing some peculiar individual difference or else they are regarded as poor introspectors. On the other hand, it is a matter quite open to debate whether the experience of pressure is really simplified in any reagent by such a procedure. It is the writer's experience that such a method only leads to a less analytical attitude on the part of the reagent, as a result of which he fails to cognize these secondary processes which do not drop out. He synthetizes them; *i.e.*, he learns to become conscious of the process as "pressure" rather than as "pressure plus"; he develops a bias toward unanalysed experience, thus preventing himself from realizing the sensory and imaginal details constituting the so-called simple meaning, pressure; just as the untrained observer possesses a bias toward the meaning, consciousness of self, and fails to realize its sensory and imaginal components. There is quite likely just as much danger of over-training as of under-training where introspection is involved.

It is the verbal-report method which leads to this over-training, this bias toward the "simple"! One never knows exactly what sort of a mental process he is dealing with, since to give a verbal report puts a premium on superficial analysis in spite of the good intentions of the experimenter and the reagent. The process appears simple for the same reason as meanings appear unanalysable to the untrained reagent. The psychology of affection, clearness and sensory qualities will undoubtedly be reinterpreted when the real complexity of these processes is generally recognized.

Our sixth point deals with the criteria of meaning. The above considerations as well as rapidly accumulating experimental evidence point to the fact that mental processes are not to be identified by structure but by function. Woods found no structural criterion for recognition; Fisher found none for the concept; Fernberger found none for the judgment; Wheeler found none for the volitional consciousness. Mosaics, cross-sections, and longitudinal sections reveal no particular uniformities, for these are to be found in function and not in content. Thus criteria of mental processes are features of behavior.

In fact, had there really been any definite structural criteria even of attributes of sensation, the literature of psychology would not have been rife with the disagreements of fact and

interpretation as to how many such attributes exist and what they are like. Likewise the problem of affection will be solved only when we shift the emphasis of our point of view from structure to function.

Two outstanding facts throw light upon the problem of criteria. First, not only affection but any given phenomenon of meaning loses its original form of existence when analysed. The meaning, task-consciousness, for example, disappears as one attends to its components, and the meanings become those of "visual imagery", "kinaesthetic sensation" and the like. Hence the reagent reports: "I was conscious of the task and this consisted of so and so," or "I experienced certain sensations and imagery which meant to me a consciousness of the task." Secondly, a given mental process is not to be identified in terms of itself, even of its parts.

Rather, the entire situation in which a process takes place must be employed in defining a certain process. A task-consciousness is defined quite as much by the situation in which it arises as by its own structural contents. The latter will in no way differentiate it from any other process. Moreover, a certain grouping of mental contents may constitute a comparative judgment in one situation and an act of choosing in another. Another group of contents may constitute a perception in one situation, an act of recognizing in another; another group may constitute an awareness of an experimental situation at one time and a task-consciousness at another time; a kinaesthetic complex may constitute a consciousness of a bodily attitude now, unpleasantness later, and so on. The manner in which these processes arise, run their course and disappear, together with their antecedent and subsequent processes, constitutes their criteria. Even the reagent's past and the environmental situation are added criteria and in many instances are the features which define the mental process in question.

For the purpose of convenience let us divide the criteria of process into three classes: (1) a function, or process-pattern; (2) a behavior-pattern; (3) a situation pattern. As an example of these various criteria we take the case of unpleasantness in a certain synaesthetic reagent. On trying to recall the name of a person whose voice is reddish-brown, our reagent experiences unpleasantness. He reports that strains appear about the brows and in the throat; eye-movements take place; the entire kinaesthesis is visualized in "moving streaks and splotches of black"; strains increase in their intensity and location. Now the black-kinaesthesis constitutes unpleasantness.

The gradual increase in intensity of "colored" strains, their persistence and irradiation constitute criterion (1), the process-pattern. But this fails to identify unpleasantness; for there is

nothing so far that will differentiate unpleasantness from muscular strain. Black-kinaesthesis may mean either the one or the other; which it will mean does not depend upon its content, or upon the immediate behavior of these contents, but upon antecedent and subsequent processes. We must thus rely upon a second criterion, a behavior-pattern.

This criterion is somewhat as follows: a definite conscious purpose has been thwarted; the reagent's recall of a name has been delayed; conflicts have developed. Owing to this situation the flow of conscious contents does not stop with the development of strain; the strains are to be *interpreted.* This interpretative consciousness may consist of a shift of attention to a certain limited region of the body which is in a tensed condition; there develops a beginning of a consciousness that the task is not being fulfilled; there follows a slight strain in the throat to pronounce a word; the word may be "unpleasant" or some equivalent. Altogether this entire behavior, patterned and conditioned by antecedent events, is our second criterion. But even yet unpleasantness has not been differentiated fully from other modes of behavior.

A third criterion remains to be ascertained. This can be found only after ascertaining the environmental conditions under which thwarted purposes lead to tensed bodily strains and organic experiences or to this or that bodily attitude. One would have to take into account the reagent's emotional predispositions. This genetic and environmental situation might be called the situation-pattern.

Is it any wonder, then, that Crosland found no specific structural pattern either for pleasantness or for unpleasantness[28] and that no specific structural patterns have been found for recognition, the concept, the judgment, the choice? None of these processes can be differentiated from any other without having recourse to the temporal relations of contents as they are running their course, to antecedent and subsequent processes, including *Kundgabe,* and to the entire situation in which the process in question is occurring. Moreover, of what psychological significance is it to reduce functional aspects of mind to mere temporal relations and shiftings in clearness, to determining tendencies of *Aufgaben,* to perseverative tendencies and the like? Do not these same phenomena mean far more when described in their natural setting of stimulus and response? Are not the real criteria of mental life behavioristic?

To state the matter briefly in another way, we have suggested three functional criteria of mental processes. (1) The process-patterns of so-called original mental contents. These are responses to physical and physiological stimuli. Moreover

[28] *Op. cit.,* 143.

they are not functions existing independently of contents; they are contents in action. Every mental process runs its course in consciousness in a certain way, terminating in a different process from the original. This criterion refers to the so-called elements of consciousness, but emphasizes them as dual or synaesthetic processes. (2) Behavior-patterns in which these dual processes are functioning. Here, in any given mental process the contents may vary from individual to individual and from time to time in the same individual while mental functions remain the same. This behavior-pattern is a "setting" involving antecedent and subsequent processes of any original content as well as simultaneous groupings of original contents. Its mechanisn of development is the motor response; hence we find that kinaesthesis is an essential "structural" component. This criterion refers to such processes as perceptions and ideas whose contents may vary but whose functional aspects do not. (3) The situation-patterns in which the behavior-patterns are functioning. Here are to be included the peculiar setting of the environment which conditions the behavior-pattern in question, the predispositions of the individual contributing to his behavior of the moment, and the response of the moment, regarded as a complex, single act. Here again the mechanism of development and action is motor response; hence we find kinaesthesis an element common to all individual cases. This criterion refers to the higher intellectual processes such as reasoning, judging and the attitudes, including the phenomena of comprehension and interpretation. Here are to be found processes whose original contents may be totally unlike except for kinaestheses, but whose functional aspects are the same. Moreover, the same processes, structurally considered, may constitute different phenomena of behavior in different situations.

These three criteria correspond to our three stages in the development of meaning; in fact they represent three levels of complexity of meaning. The first stage consists of original process-aspects of mental contents; the second stage is a behavior-pattern by means of which we "implicitly" know, and by means of which data in consciousness become data of consciousness, and further by means of which the environment becomes implicitly meaningful. Under these conditions the individual is reacting to objects and to his own actions as if they were consciously meaningful without being reflectively aware of these meanings. The third stage is the situation-pattern by means of which knowledge becomes explicit; by means of motor response the individual labels, recognizes, or defines an environmental situation, an object, or one of his own acts. Here reactions whose contents vary come to be defined by the conditions under which they take place.

Our last problem deals with a theory of meaning. We propose the behavioristic view that meaning bears the same relation to acquired human behavior as do instinct and modified instinct to animal behavior. Instinctive or modified instinctive motor responses in animal behavior are its ways of "labelling" or of "defining" its environment and its own bodily conditions. For example when a chicken sees a hawk and runs for cover, the instinct of flight "defines" the hawk as a dangerous object; flight and the attending fear, if any, are the only forms of the "meaning", hawk, apparently observable in the behavior of the chicken. During the course of human evolution, however, there gradually developed from this "lower" type of response a secondary and derived mode of reaction, superimposed upon the older and more basic instinctive type. Consequently the human being may react instinctively to a dangerous object by running from it, but he may respond intellectually as well. In either case the mechanism of defining is motor response. The human being "knows" a dangerous object by making a motor response *to* it.

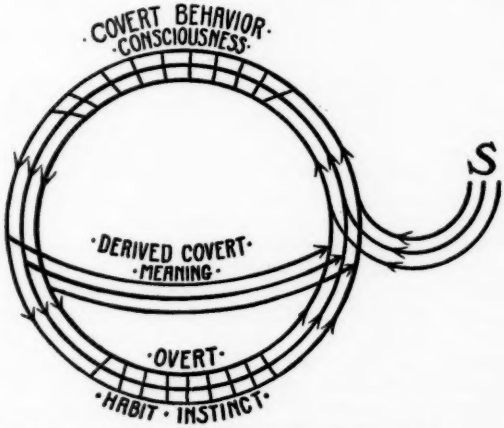


FIG. 1. SCHEME SHOWING RELATION OF MEANING TO BEHAVIOR IN GENERAL

The accompanying diagram illustrates our point. Let the circle represent the concept of the reflex arc. *S* is an external stimulus as a result of which there is set up an integration of nerve-discharges. The physiological feature of this integration is the transfer from sensory to motor discharges, while the psychological feature is covert behavior or consciousness. We assume that consciousness is no epiphenomenon or parallel

phenomenon but rather the integration itself, a synthesis. Covert behavior thus stimulated leads directly to the overt or explicit act of running for cover in the case of instinctive response.

In the case of intellectual activity, however, the original covert behavior leads to a second type of response which might be called derived or secondary action, consisting of incipient throat, facial movements and bodily attitudes. By this means there is added to or there emerges from the original covert behavior a derived covert behavior the kernel of which is kinaesthesis; and this behavior is meaning. The antecedent of this response was a previous mental state; hence the response itself is reflective, as offset against non-reflective covert behavior; it is what has been called "self-consciousness"; it is *consciousness of* anything as offset against mere consciousness.

This scheme explains the difference between perception and recognition, between mere sensory discrimination and the conscious noting of relations, between responding to a repeated feature of a class of objects and conscious noting of abstract relations. In other words, the shorter arc is the basis of intelligence,—recognition, comprehension, interpretation; the longer arc means instinct and non-deliberate habit. The second is non-intellectual and quite possibly represents a truer picture of what the Freudians mean by the subconscious.

When unanalysed, meanings as states of consciousness are different qualities of experience as truly existing as is a chemical compound, but no more existential or *sui generis* than the chemical compound. Thus the meaning, tree, is a different quality of experience from the meaning, house. The quality is a novelty of existence derived from context, synthesis, and complexity of detail, but when these meanings are analysed it is found that nothing but patterns or modes of function will identify them. As a conscious process meaning has nothing to identify it from a structural standpoint; no specific elementary content differentiates it any more than hydrogen or oxygen, as such, will differentiate water from peroxide. In other words, on their own, unanalysed level, meanings need no structural differentiation; as such they *are* different by action, not by content. When analysed down to the level of their component contents, the meanings have disappeared and hence need no structural differentiation. The important problems, then, in connection with meaning are functional and not structural; meanings are to be described, as are all phenomena, in terms of structure, but are to be identified not by contents but by modes of behavior. The best functional treatment of meaning is behavioristic, therefore, for only such a point of view seems to do justice to the facts.

THE EQUATION OF THE LEARNING FUNCTION

By Max F. Meyer and F. O. Eppright

Introduction

In spite of the fact that *the learning function is the central problem among all those problems of human and animal life which interest the psychologist*, very little has been done to describe it exactly by a suitable equation. Only one serious attempt has been made a few years ago by L. L. Thurstone,[1] who deserves much credit for trying to do this pioneer work. It is therefore necessary to state here, briefly, why I cannot follow Thurstone on the particular road which he has chosen to travel.

My objection to his procedure is that it is not based on sufficiently "rational" considerations. He says (p. 25): "As long as we do not have the basis for a rational equation for learning, I have been content to abide by it." (By a common hyperbolic equation!) Why so pessimistic? I believe that we possess much more of a rational basis than he admits.

I assert that in Thurstone's equation, $Y=f(X,L,R)$, neither the variables nor the constants have been chosen on a sufficiently rational basis. He defines Y as the number of successful acts per unit time; and X as the total number of practice acts since the beginning of formal practice. There is a very serious objection to this definition of X. It is perfectly well known, ever since Ebbinghaus if not for an even longer time, that the practice value of a particular number of practice sets varies enormously according to how they are temporally distributed. X is therefore no independent variable in $f(X,L,R)$, but both Y and X are functions of time, a fact which throws out Thurstone's definition of X in so far as it claims to be an axiomatic definition.

Even more objectionable is his definition of Y. He defines it as the number of *acts per unit time*. He has obviously been misled by the common sociological, but artificial and not fundamentally biological concept of "speed in working." A fundamental biological concept is, rather, that of the *time consumed in performing one complete and errorless act* (meaning by 'errorless' that any errors which may have occurred must have been corrected before the act is called complete).

[1] L. L. Thurstone, The Learning Curve Equation, Psych. Mon. 114, 1919, 51 pp.

A hungry cat is safe from starvation when the act of catching the mouse is complete. The completeness of the act is the primary biological concept; the speed of catching is a secondary one. No matter how great the speed of the watch and of the chase, the cat will starve if it 'almost' caught the mouse, but not quite. Even in civilized society,—the shoemaker can not make a living, no matter with what speed he has made a pair of shoes, if he tries to deliver them to his customer without the eyelets. And the tailor, no matter with what speed he has sewed the left sleeve on the right armhole and the right sleeve on the left armhole, will starve until he has corrected his error. Completeness of the task is the primary biological concept. Speed is a secondary and artificial concept. Speed cannot enter the equation as an axiomatic term.

The biological argument that we have just applied to the variable Y, applies of course also to Thurstone's first constant, L, which he defines as the upper limit of *acts per unit time.*

The second of his constants, R, is called by Thurstone the "rate of learning." This, on account of the *name chosen,* impresses the present writer as a confusing and irrational definition. R appears in his equation

$$Y = \frac{LX}{X+R} = \frac{L}{1+\frac{R}{X}}$$

as the numerator of a fraction of which X is the denominator. R, therefore, could be defined very well as that inherited property of this particular animal which determines the practice value of each of the *practice acts* (*whose number is X*) *of this animal* in comparison with the practice value of the same kind of practice (kind of task) when performed by a different animal of different heredity; that is, R could be defined as the reciprocal value of the "individual learning ability." But no mathematician, except in a moment of inattention, can assent to meaning by the "rate of learning" anything other than the first derivative of f(X,L,R), that is, than $\frac{dY}{dX}$. The "rate of learning" cannot be conceived of as one of the *constants* of the "learning" equation, unless this equation is what it undoubtedly is not, the equation of a straight line. In spite of this adverse criticism, however, Thurstone deserves very much credit for having been the first to attempt a solution of the problem.

Definition of the Primary Variables and Constants

(*t*) Let the variable "*t*" be the duration of any test (which is also practice) in which a certain finite task has been finished errorless (that is, including the correction of any errors which may have occurred).

(n) Let the variable "n" be that number of such *tests repeated at regular intervals*, which have *preceded* any certain test,—the duration of the third test, for example, being that t which corresponds to n equal to 2.

(a) Let the constant (or parameter) "a" be the "practice value" of the particular test in comparison with any other kind of "practice" which the individual may undergo during each of the regular intervals aforementioned, or in comparison with another and independent test series, where another task, larger or smaller than this, will of course on account of its magnitude have a larger or smaller practice value than the practice value "a."

(b) Let the constant (or parameter) "b" be *a certain fraction of* the "practice value" of any other kind of "practice" which the individual may undergo during each of the regular intervals in addition to the test-practice "a." By *a certain fraction* is meant that fraction of its practice value which influences, not only the practice "b," but also the outcome of the test "a." The influence of this fraction "b" may be either positive or negative according as this other kind of practice may have been chosen to be *helpful* toward the practice "a" or intentionally to *interfere* with it through building up habits opposed to those of the test-practice "a."

Let "c" etc. be those fractions of the practice value of any further kinds of practice which are intended and expected to be helpful or harmful to progress in the "a" test.

If there is no practice other than the test practice, then b, c, etc. are each equal to zero.

(p_a) Let the constants (or, rather, parameters) "p_a, p_b, p_c, etc." be the measure of the combined influence of all our instincts predetermined by heredity and of all that previous life experience of ours which is capable of extending its influence to any of the specific learning tests having the nature and practice value called "a, b, c, etc.," respectively.

The value of "p_a" can then be regarded as the *equivalent* (*positively or negatively*) of a certain number of regularly repeated practice units "a" exerting their influence on the outcome of the actual "a" tests. That is, it amounts to the same as if so many tests had been taken before the first actual and formal test which occurred at the time $n=0$. That is, again, to n will have to be added p_a before multiplying by the practice value "a" of each of these tests.

(p_b) Let the constant "p_b" be regarded as the equivalent of a certain number of formal practice periods devoted to the "b" practice, which actually have not occurred before the time $n=0$. That is, to n will have to be added p_b before multiplying by the practice value "b" of each of the "b" tests (practice periods).

Let "p_c, etc." be defined correspondingly.

$$(n+p_a)a+(n+p_b)b+(n+p_c)c \text{ etc.} =$$
$$=n(a+b+c \text{ etc.})+ap_a+bp_b+cp_c \text{ etc.}$$

In case there is no formal practice of the nature "b," "c," etc., then "b," "c," etc. are severally zero. And the above measure reduces to $(n+p_a)a$, which may be written simply $(n+p)a$.

In the new-born baby, if we could and would subject him to formal learning tests of such make-up as to be *an absolutely simple*(*and therefore also purely abstract, unreal*) *sensory-motor task to be learned*, p_a would by definition be negative, since all true learning[2] consists then and thus, never in improving a substitution already forming, but first in breaking down the inherited reaction. In *concrete* tests of formal learning the probability that

[2]True learning means "substituting a different reaction." Compare the author's *Psychology of the Other-One*, 1922, 119.

p_a is more and more negative corresponds, leaving out of the question the make-up of the tasks, to the greater and greater youthfulness of the animal tested.

(O) Let the constant (or parameter) "O" be the "original" duration of the test. This duration would depend on the individual's "original nature" (if such a test could have been given previous to and without any and all life experience). Of course, the value of O is a purely theoretical quantity, never to be directly observed in any test unsuitable to new-born babies. O corresponds to the condition which philosophers of former centuries used to call "tabula rasa." O is also equal to the product LT. See below.

(T) Let the constant (or parameter) "T" be the limit which the duration of a test approaches without ever reaching it, no matter how large n becomes.

(L) Let the constant (or parameter) "L" be the "learning ability" with which the individual has been equipped by heredity; that is, the factor by which O must be divided in order to yield T. O is therefore the product LT. The number L is most likely to be in the neighborhood of or less than 10 (as experience seems to indicate) and of course more than 1.

Determination of the Learning Function in its Primary Form

It seems rational to assume that the function F, which is yet to be determined, of the variable t and the parameters L and T should be proportional to the sum of all the products $(n+p_a)a$, $(n+p_b)b$, $(n+p_c)c$, etc. This is provisionally expressed by writing the following equation:

$$\text{I. } F(t,L,T)=F[f(t,L,T)]=n(a+b+c \text{ etc.})+ap_a+bp_b+cp_c \text{ etc.}$$

Our world experience seems to show, further, that the value of the subfunction f, which will later be determined as a function of t, L and T, is such a function of F, $f=\varphi(F)$, that the variable, f, for an increasing F in $\varphi(F)$, *drops from a finite value*, O, *to a finite value*, T, first at an ever increasing rate, later, after *passing through an inflection point*, at an ever decreasing rate. The value of $f=\varphi(F)$ seems to be asymptotic to the limits O and T aforementioned.

The inflection point is rationally demanded by the universal testimony in the beginning of learning a difficult performance that "I shall never learn that," and at a later stage that "I am surprised at the progress I am making." It goes without saying that at a still later stage this progress becomes again inconspicuous as it was at first.

But the inflection point is rationally demanded even more by our world experience that nothing "occurring" ever occurs abruptly, but that everything that starts to change starts with a change which is zero. And everything that ceases to change, ceases with a change which is zero. Example: a train leaving one station in order to stop at another. What "occurs" is that the train at some time is found out of the station. At the moment of starting to change position its velocity is never any-

thing but zero, and at the moment of ceasing to change position its velocity is again zero. The mathematical physicist asserts that discontinuous functions do not exist in nature.

Such a function $\varphi(F)$ is, for example, the *arc of a cotangent* if that cotangent, F itself, varies from *minus infinity* to *plus infinity.*

We therefore write $f=\varphi(F)$ in the form $f(t,L,T)=\text{arc cot } F$ and therefore

$$F[f(t,L,T)]=\cot f(t, L,T)$$

and we rewrite the equation I. in the following more determined form:

$$\text{II.}\quad \cot f(t,L,T)=n(a+b+c \text{ etc.})+ap_a+bp_b+cp_c \text{ etc.}$$

Since the function F has been determined as a cotangent, the subfunction $f(t,L,T)$ is to be determined in accordance with the two conditions, first, that it becomes π when t equals the constant O, and second, that it becomes zero when t equals the constant T, which two constants have been defined as the limits of the possible duration of the test. Therefore we write

$$\text{III.}\quad f(t,L,T)=\frac{(t-T)\pi}{O-T}$$

And since $O=LT$, we derive from equation II.

$$\cot\frac{(t-T)\pi}{(L-1)T}=n(a+b+c \text{ etc.})+ap_a+bp_b+cp_c \text{ etc.}$$

$$\text{IV.}\quad \cot\frac{(\frac{t}{T}-1)\pi}{L-1}=n(a+b+c \text{ etc.})+ap_a+bp_b+cp_c \text{ etc.}$$

The equation IV. is the equation of the Learning Function in its primary form.

In order to find from any series of experimental data the constants or parameters whose discovery is the purpose of the experiment, it is necessary to transform equation IV. so that it can most easily be applied to the experimental data of the particular case. We shall make this transformation presently.

It seems possible to understand rationally that the inflection point — at which $(t-T)$ is one-half of $(O-T)$ and also the sum of the products $(n+p_a)a$ etc. is zero — should be reached just where this sum is zero. Until the inflection point is reached, the biological processes are such that "breaking down" prevails among the sensory-motor units of function. After the inflection point, "building up" prevails. (For a hint as to the real significance of these two stages, "breaking down" and "building up," compare the discussion of the two stages of learning in the author's *Manual of Psychology Demonstrations*, 1922, 10.)

Numerical Determination of the Parameters p, L, T *and* a *by Plotting*

The most convenient method (requiring no greater mathematical knowledge than reading a trigonometric table) of finding the constants of the Learning-Curve of any experiment consists in plotting the experimentally found values of t against n on a sufficiently large sheet of coordinate paper and then plotting the computed values of t for various constants until one is satisfied that, but for accidental variations, the computed curve is identical with the experimental curve. An example of this method will be given below.

The preliminary thing necessary, then, is to solve equation IV. for t. This is easily done and yields as result equation V. For simplicity's sake we choose an experiment in which b, c, etc. are severally equal to zero, that is, in which there is no practice other than that of the repeated tests; and in which the constant a may be made "unit," that is, in which no comparison is made with another experimental series setting a different task for the test, and whose task then might, and probably would, have a different practice value.

$$\text{V.} \quad t = \mathrm{T} + \frac{(\mathrm{L}-1)\mathrm{T}}{\pi} \operatorname{arc\,cot}(n+\mathrm{p})$$

It is convenient to substitute K for that factor by which the arc cot is multiplied.

$$\mathrm{K} = \frac{(\mathrm{L}-1)\mathrm{T}}{\pi}$$

$$\text{VI.} \quad t = \mathrm{T} + \mathrm{K} \operatorname{arc\,cot}(n+\mathrm{p})$$

Below is given a table of experimental data consisting of the records of a test made on the present writer during the spring of 1922. The experiment was not at all an experiment on learning, but on the effects of certain drugs taken day after day in a certain rotation. That is the reason why the data in the table are arranged in four columns. However, irrespective of the influences of the drugs which may be regarded as accidents, the data are the records of the time necessary to read and rewrite in shorthand a certain definite and constant list of words presented in shorthand.

SUGAR			TEA			ALCOHOL			TOBACCO		
Date	t	n	Date	t	n	Date	t	n	Date	t	n
May 26	5503	0	May 27	4424	1	May 28	4331	2	May 29	3974	3
May 30	3371	4	May 31	2945	5	June 1	2961	6	June 2	2725	7
June 3	2712	8	June 4	2536	9	June 5	2594	10	June 6	2470	11
June 7	2643	12	June 8	2411	13	June 9	2333	14	June 10	2295	15
June 11	2203	16	June 12	2253	17	June 13	2325	18	June 14	2117	19
June 15	2158	20	June 16	2110	21	June 17	2102	22	June 18	1972	23
June 19	1985	24	June 20	2048	25	June 21	2143	26	June 22	——	27

In order to find now by plotting the parameters T, L, and p for this experiment, we proceed as follows. First we prepare a large enough sheet of paper with eight blank columns and a large number of lines. (See Computation Table I.)

The first column from the left we call n. It will contain the ordinal numbers, 0 for the first test, 1 for the second test, 2 for the third test, and so forth. The second column will place before our eyes the constant value of p as we guess at it with increasing approximation to its true value. The third column will contain the sum of n and p. The fourth column is open for our entries of the arc belonging to that cotangent which is equal to the just mentioned sum in the preceding column. It is most convenient to use trigonometric tables which give the arc in degrees and minutes. We write down the degrees immediately, then place a decimal point, and find the next two decimals by dividing, mentally, the minutes of the table by 6. The fifth column serves to place before our eyes the constant K guessed at with approximation gradually increasing. The sixth column receives the product of the numbers contained in the fourth and fifth. The use of Crelle's *Calculating Tables* is to be recommended for this purpose. The eighth column receives the values found experimentally as corresponding to the various values of n. Now we have to scrutinize columns six and eight in order to choose a value of T which, when added to the sixth column, will give the best possible approximation (entered in the seventh) to the somewhat irregular values of the eighth column.

Successive Approximations

(1)	p=2.0,	K=100,	T=1765.
(2)	p=2.0,	K=200,	T=1545.
(3)	p=2.5,	K=200,	T=1551.
(4)	p=2.5,	K=220,	T=1508.
(5)	p=2.5,	K=180,	T=1670.
(6)	p=2.4,	K=180,	T=1700.

Now we guess, for using our example of experimental records, first at p=2.0 and at K=100. We choose then from our table of experimental records either the extreme values of n or some values near the extremes, say, $n=2$ and $n=24$. In the eighth column we may at once write the corresponding values of t found during the experiment, *i.e.*, 4331 and 1985. The computation gives us (1) for the sixth column 1433 and 220. Choosing 1765 for T, we get for the seventh column 3198 and 1985.

COMPUTATION TABLE I.

n	p Constant	$n+p$	arc cot$(n+p)$	K Constant	K arc cot	+T	Experimental t
(1) 2	2.0	4.0	14.33	100	1433	3198	4331
24		26.0	2.20		220	1985	1985
(2) 2	2.0	4.0	14.33	200	2866	4411	4305
24		26.0	2.20		440	1985	1985
(3) 2	2.5	4.5	12.53	200	2506	4057	4305
24		26.5	2.17		434	1985	1985
(4) 0	2.5	2.5	21.80	220	4796	6304	5503
2		4.5	12.53		2757	4265	4305
6		8.5	6.71		1476	2984	2961
12		14.5	3.94		867	2375	2643
24		26.5	2.17		477	1985	1985
(5) 0	2.5	2.5	21.80	180	3924	5594	5503
2		4.5	12.53		2256	3926	4305
4		6.5	8.75		1575	3245	3371
6		8.5	6.71		1208	2878	2961
12		14.5	3.94		709	2379	2643
20		22.5	2.55		459	2129	2158
24		26.5	2.17		391	2061	1985
(6) 0	2.4	2.4	22.62	180	4070	5770	5503
2		4.4	12.80		2304	4004	4305
4		6.4	8.88		1598	3298	3371
6		8.4	6.78		1220	2920	2961
8		10.4	5.49		988	2688	2712
12		14.4	3.98		716	2416	2643
16		18.4	3.10		558	2258	2203
20		22.4	2.55		459	2159	2158
22		24.4	2.35		423	2123	2102
24		26.4	2.17		391	2091	1985
26		28.4	2.02		363	2063	2143

We notice that the range of the seventh column is insufficient. We therefore try a *greater* K, taking it this time as equal to 200. Computation (2) then yields for the sixth column 2866 and 440. Choosing 1545 for T, we get for the seventh column *a greater range*, 4411 and 1985, which is about the range we need. K, therefore, must lie in the neighborhood of 200, unless our guess at p was very far off the mark.

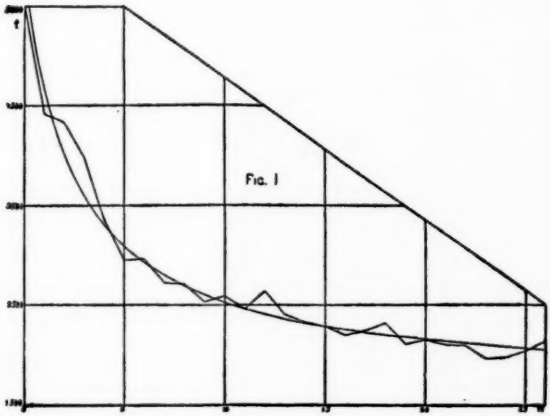
Let us see what difference it will make if we choose a slightly different p. We take p a little *larger*, equal to 2.5, and computation (3) yields for the seventh column a slightly *smaller range*, from 1985 to 4057.

Let us now (4) keep p equal to 2.5 and take K still a little *larger*, equal to 220, at the same time making the computation for five n's instead of two only. We now plot on a sheet of coordinate paper the experimentally found data consisting of the twenty-seven pairs of values for t and n. On the same

paper we enter the five values for t just found by computation, 6304, 4265, 2984, 2375, 1985. If the reader will do this, he will convince himself that *the left branch* of the computed curve in the neighborhood of $n=0$ appears now to *fall too steeply*. In order to make it *less steep*, we might try to *reduce* K somewhat. Let us reduce it to 180.

We now compute (5) for seven judiciously chosen values of n the corresponding t. Plotting the values of t computed we observe that the left branch of the curve in the neighborhood of $n=0$ now has a better direction or bearing, but that it does not yet fall steeply enough. Nevertheless, we hesitate to reduce K still more, since the range of t is now pretty good, and we know already that an increase of K has a great effect on increasing the range. We therefore try to get still a little *more steepness* in the neighborhood of $n=0$ by *reducing* p *a little*.

We now take K=180 and p=2.4 and compute (6) for eleven values of n the corresponding values of t and plot them.



The illustration shows how the curve of which they are points (*i.e.*, the smooth curve) passes among the experimentally found values of t. He who desires a still better approximation may let himself be guided by a rule which usually works out well, that *when the approximation is already pretty good and it is desired to make the curve more curved, more hollow, one should reduce both* p *and* K *a little. And to make the curve straighter, shallower, one should increase both* p *and* K *a little.* (Anyone interested in

knowing why that is so, should study the mathematical properties of the cotangent.) Always last adjust T to suit that part of the experimental curve which runs most nearly horizontal.

As soon as we are satisfied with the values of p, K and T (for example with $p=2.4$, $K=180$, $T=1700$), we find L by reference to the substitution formula directly preceding equation VI.

$$L=1+\frac{\pi K}{T}=1+\frac{180\cdot 180}{1700}=1+\frac{324}{17}=1+19.1=20.1$$

Another Method for Determining the Constants p, L *and* T

(By F. O. Eppright)

It may be readily seen by inspection of equation VI. that if t is plotted against the value of arc cot $(n+p)$ the resulting curve will be a straight line provided the correct value of p is chosen. This, of course, is true only if the empirical values of t vary approximately as this function of n. The fact that when the proper value of p is chosen this curve is seen to be approximately a straight line, or rather, a straight ribbon, is itself a justification for using, as a function of t and n to represent the learning curve, the kind of function we have used.

In plotting the values of t against the values of arc cot $(n+p)$ it is convenient to let $\alpha=$arc cot $(n+p)$. Then equation VI. may be written $t=K\alpha+T$. Now, when the correct values of α have been determined for the corresponding values of t, we get a more or less ragged ribbon as is shown in Figure D. K is then equal to the ribbon's slope, and T is equal to the ordinate value of that point at which the line of the ribbon, if extended, cuts the t-axis.

The procedure for determining the value of the constant p which will give the proper value of α is as follows. From the table of experimental records shown the writer chose three pairs whose n-values were 0, 13 and 44. Three points were chosen because at least three points are required to determine a straight line as distinguished from a curve. It is advantageous to choose one of these points near the beginning, one from near the end and one about one-third the way from the beginning to the end of the experiment.

TOBACCO		SUGAR		TEA		ALCOHOL	
n	t	n	t	n	t	n	t
0	1977	1	1790	2	1796	3	1724
4	1671	5	1552	6	1498	7	1676
8	1509	9	1410	10	1421	11	1553
12	1386	13	1411	14	1449	15	1392
16	1312	17	1360	18	1333	19	1291
20	1254	21	1228	22	1253	23	1244
24	1242	25	1181	26	1199	27	1201
28	1132	29	1157	30	1127	31	1143
32	1127	33	1124	34	1105	35	1138
36	1091	37	1115	38	1053	39	1075
40	1078	41	1094	42	1035	43	1064
44	1024						

Rule columns as shown in Computation Table II. and write in the selected values of n and the corresponding values of t. Then arbitrarily assign some value to p, for instance 1, and fill in the column of the values for $n + p$, in this case, 1, 14 and 45. By the aid of a trigonometric table of cotangents determine the corresponding values for α and write them in the proper column. Now plot on coordinate paper α against the values of t, that is, 1977, 1411, 1024. The three points thus located will form a triangle whose obtuse angle will open toward the right as in Figure A. This is due to p having been chosen too small.

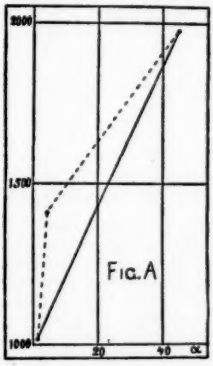


FIG. A

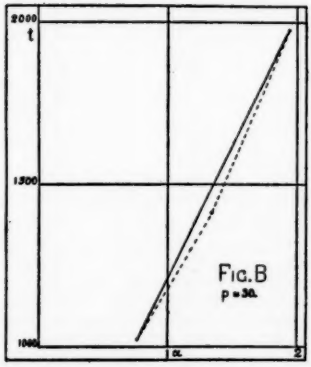


FIG. B
p = 30.

We now try the same three values of n, 0, 13 and 44, for p = 30. This value of p has been taken too large as is shown by the fact that the obtuse angle opens toward the left as in Figure B. We now try a value for p which lies between 30 and 1, for example 18. By taking pains with further approximations of p we can make the three points of the angle lie as nearly in a straight line as may be desired. To make them lie exactly in line would ordinarily be a useless procedure, because the best that can be expected of the remaining points not yet located is that they will fall within a straight and reasonably narrow path whose direction has been indicated by the three points already placed. It happens here that p = 18 gives a sufficiently straight path as indicated in Figure C.

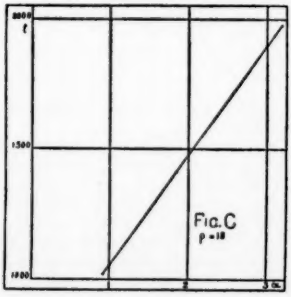


FIG. C
p = 18

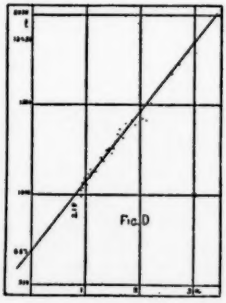


FIG. D

The value of p having been approximately determined by experimenting with three points, it is now well to calculate the remaining values of α and plot them as shown in Figure D. The path now containing more points, it will be easier to decide whether or not its trend is straight, and if necessary a further correction of the value of p can be made.

COMPUTATION TABLE II.

n	p	$n+p$	α=arc cot	t
0	1	1	45.00	1977
13		14	4.08	1411
44		45	1.27	1024
0	30	30	1.91	1977
13		43	1.33	1411
44		74	.77	1024
0	18	18	3.18	1977
13		31	1.85	1411
44		62	.92	1024

In the writer's opinion it is well to stop with the *smal'est* value of p that will give a reasonably straight path. It may be expected to flare somewhat at the upper end. The *width* of the path is not particularly significant in choosing the value of p. A broad path merely means that the experimental values of t have deviated considerably both above and below the theoretical values as determined by the equation. After the path has become straight a further increase in the value of p may cause the path to become narrower, but it will not make the theoretical values of t approach any more closely the experimental values. On the contrary, it will cause the difference between them to become greater, which is obviously not to be desired.

After a suitable value of p has been determined in this manner, the values of K and T might be calculated by the method of Least Squares. This method is more or less time-consuming in that it requires a great deal of computation. There is also a difference of opinion in regard to its superiority to the method of inspection; but it does have the advantage that different computers will get identical results from the same data. For the convenience of anyone wishing to try this method we reproduce here the formula used by the mathematicians, in which "r" is equal to the number of pairs of values of t and α used for the computation.

$$T = \frac{\Sigma t_1 \Sigma \alpha_1^2 - \Sigma \alpha_1 \Sigma \alpha_1 t_1}{r \Sigma \alpha_1^2 - (\Sigma \alpha_1)^2}$$

$$K = \frac{r \Sigma \alpha_1 t_1 - \Sigma \alpha_1 \Sigma t_1}{r \Sigma \alpha_1^2 - (\Sigma \alpha_1)^2}$$

The writer therefore recommends the use rather of the method of inspection, the procedure for which is as follows.

After the points have been plotted on a rather large scale, hold a string stretched tightly between the two hands in that position which seems to coincide best with the general bearing of the points. Then draw a straight line to take the place of the string, as, for instance, in Figure D. Extend it to cut the t-axis. The ordinate value of this point is the value of T,— in this instance it is 687. To determine the value of K, select two convenient points on the line a considerable distance from each other, and by reading *from the scale of the drawing* determine their vertical and horizontal dis-

tances apart as indicated by the marked lines in Figure D. The value of K is equal to the vertical distance between the two points divided by their horizontal distance,— in this instance K is equal to 386. (This high quotient looks as if it referred to an almost vertical line. That the line in Figure D is not nearly vertical is due to the fact that the horizontal scale is greatly exaggerated in comparison with the vertical.) From K we easily find L.

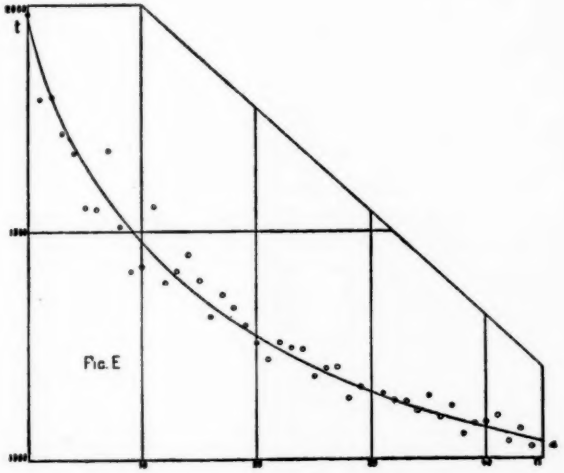


Figure E shows how the computed curve runs among the experimentally found dots.

If these constants are to be determined for several sets of data, the writer has found it desirable to make tables giving the values of α for a series of values of $n+p$. This is because the ordinary trigonometric tables give the values of the cotangents to degrees and minutes, but not to seconds. In consequence the value of the cotangent sought is seldom given in the table very exactly. It more often gives values lying considerably on each side of the one wanted. For example, if we wish to determine the angle whose cotangent is 61.0, we find that the table gives 61.3829 for the cotangent of no degrees and 56 minutes, and 60.3058 for the cotangent of 57 minutes. To find the angle whose cotangent is 61.0 we have to interpolate by substracting 60.3058 from 61.3829. This gives 1.0771, which is the amount the cotangent decreases as the angle increases from 56 to 57 minutes. But in order to reach the value of 61.0 the value of 61.3829 would have to decrease 0.3829, which is about 0.36 of 1.0771. Correspondingly the angle of 56 minutes would have to increase 0.28 of one minute. This gives us the angle of 56.36 minutes as the angle whose cotangent is 61.0. This value in minutes must now be converted to decimal parts of a degree by dividing 56.36 by 60, which gives us 0.94 degrees, the angle sought. It is therefore obvious that if these values are used more than once it is economical of both time and effort to arrange them in tabular form as is illustrated in the samples shown below.

FOR DECIMAL .0		FOR DECIMAL .5	
$n+p$	α=arc cot	$n+p$	α=arc cot
0.0	90.00	0.5	63.43
1.0	45.00	1.5	33.36
2.0	26.56	2.5	21.48
3.0	18.43	3.5	15.95
4.0	14.33	4.5	12.53
5.0	11.31	5.5	10.30
etc.		etc.	

We need only tables in which the successive values of $n+p$ differ by unity. And since the p-values need never be chosen more exactly than including the first decimal, we need only ten such tables. The writer has made these ten tables for the decimals .0, .1, .2, . . . to .9, each table extending from zero units to 65 units of $n+p$. He will be glad to send mimeographed copies of them to those wishing to use them.

Definition of the Secondary Variables and Constants

Next to the *duration* of one act which, when complete, fills a biological need,— the *average speed* at which the interval between two certain moments of *time is filled by a multitude* of homogeneous and complete acts, is a factor of biological importance. For example, a society of bees which, during the season when plants bloom, can fill a larger number of cells with honey, establishes thereby a greater survival probability for itself (*i.e.*, both the living individuals and the future offspring). Another example: the farmer who during the season when corn planting is possible — which in some climates is limited to a few weeks of the year — can place a larger number of seeds properly in the soil, is less likely to starve from one harvest to the next.

(s) Let the variable "s" be the *average speed* at which a certain test (and practice) period has been filled by a multitude of homogeneous and complete acts of the nature and practice value "a" each.

(n) Let the variable "n" be that number of such test periods which, at regular intervals, have preceded any certain test,— the average speed of the third test, for example, being that which corresponds to n equal to 2.

(Θ) Let the constant "Θ" be that duration which has been chosen as convenient for each and every test (and practice) period of the experiment. The product of Θ and s is then obviously the equivalent to "the practice value" as primarily defined; with this difference, however, that now the practice value of the work done during a test period is a variable, "Θs," and not, as formerly, a constant, "a," of the equation.

(σ_a) Let "σ_a" be the *variable* number which indicates how many times that work unit whose size and nature is "a" can be (and is) performed at any of the speeds "s" during the constant test (practice) period lasting Θ time units. Briefly, it is *the amount per* Θ and depending, then, of course on n.

Since σ_a thus becomes the measure of *the quantity of practice*, it is obvious that it must, in this secondary form of the equation, take the place of "a" as used in that primary form.

It is also obvious that Θs can be *substituted* for σ_a, since the number σ_a divided by the duration Θ must be the speed s.

$\frac{\sigma_a}{\Theta}=s$ and therefore $\Theta s=\sigma_a$.

(σ_b) The variable number "σ_b" (depending on n) shall indicate the product of two factors, one being the fraction of "b" either helpful or harmful to progress in "a," the other factor being the number of (with n variable) times the task "b" can be performed at any of the speeds "s" during the test (practice) periods lasting Θ.

But we cannot substitute for σ_b anything more definite without a special experimental investigation of the varying speed of "b."

Further, "σ_c" shall indicate the correspondingly understood variable product for the task "c." And so forth for other terms of the kind σ.

(N_a) Let "N_a" be the constant which indicates the number of imaginary formal practice periods devoted to "a" which, if instead of his previous life experience they had preceded $n=0$, would have enabled the individual to work at the task "a" with the same speed with which he really does work at the task "a" when n is equal to zero.

(N_b) Let "N_b" be the constant which indicates the number of imaginary formal practice periods devoted to "b" which, if instead of his previous life experience they had preceded the moment $n=0$, would have enabled the individual to work at the task "b" with the same speed with which he does work at the task "b" when n is equal to zero.

Let "N_c, etc." be defined correspondingly.

(Ω) Let the constant "Ω" be the "original" speed. It is a purely theoretical quantity (like the former "O"), never to be directly observed. It corresponds to the philosophers' "tabula rasa."

(S) Let the constant "S" be the limit which the average speed per working period approaches without ever reaching it, no matter how large n becomes.

(L) Let the constant "L" be an hereditary factor having the same meaning as our formerly defined "individual learning ability." That is, the reciprocal value of Ω is equal to L times the reciprocal value of S. And $\Omega L = S$.

Determination of the Learning Function in its Secondary Form

It seems rational to substitute in equation III. $\frac{1}{s}$ for t, $\frac{1}{S}$ for T, $\frac{1}{\Omega}$ for O, and in equation IV. σ_a or Θs for a, σ_b for b, σ_c for c, etc., N_a for p_a, N_b for p_b, N_c for p_c, etc.

We thus obtain

$$\frac{(\frac{1}{s}-\frac{1}{S})\pi}{\frac{1}{\Omega}-\frac{1}{S}}=\frac{(\frac{1}{s}-\frac{1}{S})\pi}{(L-1)\frac{1}{S}}$$

and further (compare equation II. in order to see the reason)

$$\cot\frac{(\frac{1}{s}-\frac{1}{S})\pi}{(L-1)\frac{1}{S}}=n(\sigma_a+\sigma_b+\sigma_c \text{ etc.})+\sigma_a N_a+\sigma_b N_b+\sigma_c N_c \text{ etc.}$$

VII. $$\cot\frac{(\frac{S}{s}-1)\pi}{L-1}=n(\Theta s+\sigma_b+\sigma_c \text{etc.})+\Theta s N_a+\sigma_b N_b+\sigma_c N_c \text{ etc.}$$

The equation VII. is the equation of the Learning Function in its Secondary Form. But a glance at the equation, if we remember the definitions agreed on, is sufficient to convince us that, exactly as it stands there, we shall never have any use for it. It must be simplified for the following reasons, which are obvious enough. The coefficients σ_b, σ_c, etc. are not constants, but are themselves functions of n similar to the function $s=f(n)$, but differing from it at least in their own constants. And to discover the constants of these functions σ_b, σ_c, etc. experimentally, too, while experimenting on finding the constants of the main function $s=f(n)$, seems to be a task too complicated for mere human intelligence.

It is a different matter, however, if we restrict our experiment to a single kind of practice, leaving out all other kinds of formal practice of a helpful or harmful nature to the test practice. Then σ_b, σ_c, etc. are all zero, and equation VII. reduces to the following more comfortable equation:

$$\text{VIII.} \quad \cot \frac{(\frac{S}{s}-1)\pi}{L-1} = (n+N_a)\Theta s,$$

in which we might just as well write simply N for N_a.

This is the only equation of the Learning Function in its Secondary Form for which we shall have any use in psychological experiments,— that is, this equation VIII. or one to which it reduces by regarding any one of its four constants as an absolute constant, a fixed number, and only the other three as parameters which may be differently selected in the experiment.

For example, if we have correctly interpreted the meaning of the constant L, then, by performing all the experiments on the same individual, that individual's *learning ability*, L, should be found to be the same number in all the experiments. Experimental work still to be undertaken will have to prove or disprove this point.

Or, if every test (practice) period lasts invariably half an hour, Θ may as well be called "unit" and our equation would be:

$$\text{IX.} \quad \cot \frac{(\frac{S}{s}-1)\pi}{L-1} = (n+N)s$$

The equation IX. is that equation of the Learning Function in its Secondary Form which will be most useful to most experimenters, for generally they will select a definite number of minutes as a convenient test (practice) period to be adhered to throughout their experimenting.

Numerical Determination of the Parameters N, L, S *and* Θ *by Plotting*

The method of finding the constants of the Learning Curve by plotting is much less convenient in the secondary form of the equation than in its primary form. That is reason enough why *experimenters should shun as much as possible the secondary form* and plan their experiments so that working efficiency is measured by *keeping a record of the durations*, and not, as here, of the speeds.

The inconvenience is due to the fact that such an equation as VIII. or IX. is *by no analytical method* solvable for s. It is solvable only for n.

$$\text{X.} \quad n = -N + \frac{1}{\Theta s} \cot \frac{(\frac{S}{s} - 1)\pi}{L - 1}$$

When Θ is called "unit," the equation X. becomes of course a little simpler, since the cotangent is then to be divided simply by s instead of by Θs.

Below is given a table of experimental data consisting of the records of tests made by Mr. F. O. Eppright on himself. The experiment was not at all an experiment on learning, but on certain drug actions. It is as such a further typical example of an experiment ideally unrelated to learning which cannot actually be performed without taking into account the learning curve. The data are arranged in four columns in order to make it easier to see which of them were influenced by this or that drug. But the drug effect will not be discussed here, but in a different publication. The value of s in the table shows the number of words of a small and supposedly known vocabulary translated during $\Theta = 30$ minutes from one language into another and written down. The drug effect, left out of consideration here, is mentioned here only as one among many factors making the experimental curve irregular.

ALCOHOL			TOBACCO			SUGAR			TEA		
Date	s	n	Date	s	n	Date	s	n	Date	s	n
May 28	123	0	May 29	130	1	May 30	138	2	May 31	142	3
June 1	143	4	June 2	151	5	June 3	149	6	June 4	165	7
June 5	170	8	June 6	175	9	June 7	182	10	June 8	184	11
June 9	173	12	June 10	193	13	June 11	200	14	June 12	200	15
June 13	194	16	June 14	203	17	June 15	207	18	June 16	209	19
June 17	203	20	June 18	207	21	June 19	212	22	June 20	217	23
June 21	212	24	June 22	221	25	June 23	226	26	June 24	219	27
June 25	214	28	June 26	—	29	June 27	—	30	June 28	—	31

In order to find now by plotting the parameters S, L and N for this experiment, we proceed as follows. First we prepare a large enough sheet of paper with nine blank columns and a large number of lines. (See Computation Table III.)

The first column from the left we call s. It will contain the records successively taken in the experiment. The second column is open for our entries computed through dividing S by s. The third column will contain the value of the second column diminished by 1. The fourth column will place before our eyes the constant value of π divided by $(L-1)$, taking as the value of π the number of degrees, 180. The fifth column will receive the product, with not more than three places after the decimal point, of the third and the fourth entry. It goes

COMPUTATION TABLE III.

	s	$\frac{S}{s}$	$\frac{S}{s}-1$	$\frac{\pi}{L-1}$ Constant	Product	cot	$\frac{\cot}{s}$	$\frac{\cot}{s}-N$	Experimental n
(1)	.123	2.44	1.44	20.00	28.8	1.819	14.8	0.0	0
	.214	1.40	.40		8.0	7.115	33.2	13.2	28
(2)	.123	2.44	1.44	10.00	14.4	3.895	31.7	0.0	0
	.214	1.40	.40		4.0	14.301	66.8	35.1	28
(3)	.123	2.60	1.60	10.00	16.0	3.487	28.4	0.0	0
	.214	1.50	.50		5.0	11.430	53.4	25.0	28
(4)	.123	2.60	1.60	9.47	15.152	3.693	30.0	0.0	0
	.142	2.25	1.25		11.837	4.773	33.6	3.6	3
	.149	2.15	1.15		10.890	5.201	34.9	4.9	6
	.175	1.83	.83		7.860	7.237	41.4	11.4	9
	.173	1.85	.85		8.049	7.071	40.9	10.9	12
	.203	1.58	.58		5.493	10.381	51.1	21.1	17
	.226	1.42	.42		3.977	14.33	63.4	33.4	26
	.219	1.46	.46		4.356	13.12	59.9	29.9	27
	.214	1.50	.50		4.735	12.07	56.4	26.4	28

without saying that for all divisions and multiplications we have Crelle's *Calculating Tables* on hand. The sixth column will receive the cotangent of the product just mentioned, as far as three decimal places. We look up the cotangents in any small trigonometric table of degrees and minutes (seconds are not needed). In order to use the tables, we have to translate the decimals of the last column's product from hundredths into sixtieths, that is, into minutes. This need not be done in writing. Mental arithmetic suffices since it requires only multiplying the decimals by 6 and rounding off the minutes thus found. The seventh column stands ready to receive the cotangent divided by the number of the first column. The eighth column, being the quotient just mentioned minus N, tells what the value of n ought to be if this s had been the true s, free from accidental variations. And the ninth and last column gives us the actual value of n in the experiment.

Now we guess at what might be suitable numbers for S and L. Before this, however, we ought to decide what would be a convenient measure of the "unit" of s. This unit ought to be chosen so that in looking up the cotangent we can use the most convenient part of the trigonometric table, which is that between angles of about 3 and 30 degrees. For this reason, in our case, 1000 words per Θ would be a convenient speed unit. Instead of 123, for example, we shall therefore write s equal to 0.123.

Successive Approximations

(1)	S=0.30,	L=10.0,	N=14.8.
(2)	S=0.30,	L=19.0,	N=31.7.
(3)	S=0.32,	L=19.0,	N=28.4.
(4)	S=0.32,	L=20.0,	N=30.0.
(5)	S=0.32,	L=17.2,	N=25.5.

We reached the highest record in our experiment on June 23, as our record table here reproduced shows, with 226 words after having started on May 28 with the low record of 123 words. It is therefore a fairly good guess that the limit S will lie in the neighborhood of 300 words. It is more difficult to guess at L. Let us use for a first approximation L=10 and compute *n for only the first and the last experimental record.* (See Computation Table III.) We thus get for the seventh column, cotangent divided by s, the numbers 14.8 and 33.2. Choosing 14.8 as our first approximation of N, we have for the eighth column 0.0 and 13.2, instead of the experimental values 0 and 28.

Since the range of n obtained with the above values of S=0.30 and L=10.0 is too small, we try if L=19 will give a better approximation. In this case the constant quotient π divided by (L−1) is not 20.0, as it was the last time, but only 10, as entered in the fourth column. The seventh column suggests to us to try N equal to 31.7. And the eighth column gives us then 0.0 and 35.1 instead of the true values 0 and 28.

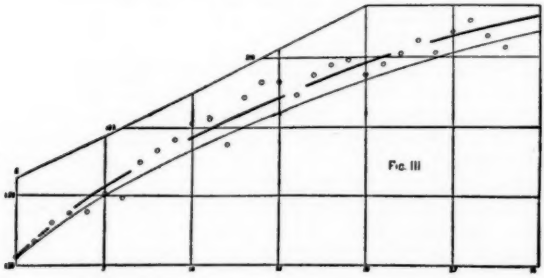
This time the range of n turned out a little too large. We now try out what difference it will make if we leave L equal to 19, as we just used it, but take S a little larger, say, equal to 0.32. The eighth column then gives us 0.0 and 25.0 This time the range of n obtained with the larger S is smaller, but only a trifle smaller than the quantity we want. We might try to remedy this in the manner which we have already discovered, that is, by taking L still a little larger.

Let us, then, this time, try out what approximation we get by taking S equal to 0.32 and L equal to 20.0 The constant quotient used in the fourth column is then 9.47. Let us also,

since we have already reached a considerable degree of approximation, compute n for a larger, but judiciously chosen number of the s values in the experimental record table. For example, the s values corresponding to the experimental n values 0, 3, 6, 9, 12, 17, 26, 27, 28 seem quite suitable.

The eighth column, if we choose N equal to 30.0, gives us as the result of the computation 0.0, 3.6, 4.9, etc. The second value is a little too large a number; the third, on the contrary, is too small. The fourth, 11.4, is again too large; 10.9 is too small; 21.1, 33.4 and 29.9 are too large; and 26.4 is too small. This already looks like a very admirable approximation.

Plotting this curve and marking in addition all the actual, experimental, n values corresponding to the s values, we obtain the curve and dots of Fig. III. The thin, smooth curve is the



curve of the constants last selected. Comparing its bearing with the location of all, and not merely a few, of the experimental points, we are not yet satisfied with the approximation. We therefore try still further variations of the constants by small amounts. The broken and heavy curve of the same figure, whose constants are S=0.32, L=17.2 and N=25.5, appears to be a good approximation to all the points combined. The last mentioned constants might therefore be used in the interpretation of the experiment, which, we recall, was not an experiment on learning at all. But this interpretation must be left to another article.

THE PHENOMENON OF 'MEANING'

By R. M. Ogden, Cornell University

'Imageless thought' has apparently passed into the limbo of inert conceptions, but, stillborn though it was, it had a positive influence upon subsequent developments in systematic psychology. No one, I think, who surveys the field to-day can escape the conclusion that the controversy which arose over the admission of a 'thought-element' among the contents of consciousness was settled less upon its merits than by its absorption into a broader question whether, indeed, psychology has any conscious contents at all.

Into this newer controversy which is still being debated I have no wish to enter here, but I do suggest that the obligation placed upon psychologists to re-define their contents in scientific terms first became obvious after the inadequacy of *sensations*, *images*, and *feelings* was revealed by the introspective studies of Binet, Woodworth, and 'the Würzburg School'. It was this argument over the nature of conscious contents which more than anything else, I think, created a favorable moment for the appearance of *behaviorism*, and its denial of scientific validity to any and all these phenomena of consciousness. In short, I doubt if behaviorism could have arisen if the conception of mind as an aggregate of conscious entities had not itself seemed to be breaking down. Accordingly, in venturing to rake over the ashes of an extinguished fire I am moved by the thought that the question which was then raised has not been answered, but only set aside.

While the pendulum of psychological interest in America swung abruptly from a seemingly hopeless attempt to give a useful definition of mind in terms of conscious elements, to a radically opposite attempt to re-define psychology in terms of behavior, German psychology has been continuing its inquiry into the phenomena of meaning. And now that the veil of war-time obscurity has been raised we find ourselves confronted with a new German psychology, in many respects radically differing from the old, yet still intent upon a scientific pursuit of the phenomena of experience.

It is especially from one of these newer points of view—that of the *Gestalttheorie*—that I now invite you to assist in a post-

[1]Some Experiments on the consciousness of Meaning. *Studies in Psychology: Titchener Commemorative Volume*, 1917, 79-120.

mortem of one of my own previous efforts to examine into the psychology of meaning[1].

Structural psychology formerly maintained that sensation was the chief if not the only element of consciousness. With this principle established, the inference was readily drawn that mind could be regarded as a summation of static entities held together by the gravitational influence of association by contiguity. This theory seemed to stand the test of the work on memory initiated by Ebbinghaus; and it was not until the thought-processes were in turn subjected to observation that a paucity of both sensory and imaginal material became evident. Instead of the traditional *sensation* and its counterpart the *image*, the observers of thought reported *whats* and *thats* and *ifs*, globular masses of interrelated tendencies, individual flashes of circumspection, and, indeed, a large assortment of vague and ill-defined phenomena, none of which bore any manifest likeness to the sensory, imaginal, and affective 'elements' our text-books had always taken such pains to describe for us.

When thought was thus discovered as a new star in the firmament, conservatives promptly denied the validity of the method of its discovery, and as promptly challenged the discoverers to define their new element in positive descriptive or attributive terms. This was not easy; for the thought-datum had no definite spatial or temporal setting, its intensity was doubtful, and its quality showed none of the systematic uniformities that had made the observation of sensation so engaging a task. Although the innovators themselves, I fancy, have never been quite ready to admit that their experiments were only pseudo-experiments, or that the data they observed and reported upon were but the crude manifestations of a totally unrefined methodology, still theirs was a losing fight, because they were unable to reduce 'imageless thought' to the scientific terms then current. Perhaps the method was faulty—though it seemed to be the same that had been used with such brilliant success in the observation of colors, tones, and varied types of imagery—but more seriously at fault, was the presupposition that conscious phenomena are reducible to elements. Such a presupposition worked well enough with sensory experience, where one could control the physical source, and thus reduce the content to manageable proportions. With meanings which spread all over the phenomenal content it was a wholly different matter; because apparently it was of the very nature of meanings to spread, and they had no physical counterpart which could be controlled. Thus the conservatives, fortifying themselves by reference to the exact sciences, insisted that there are no existential meanings. Is it not an axiom of the physicist that he is and must be totally unconcerned about the meaning

of matter? And if the biologist has been somewhat less certain in his complete abandonment of teleology, he can still appear to maintain a scientific detachment by employing the term 'function.'

Facing the alternative of mechanism or teleology, we have been wont to assume that there can be but two horns to this dilemma; either an out-and-out mechanism, or 'an increasing purpose,' and that we most therefore grasp the one horn and avoid the other.

It is a special merit of the new *Gestalt*-psychology, as I see it, to have destroyed the terrors of this alternative[2]. By a bold declaration that there are no elemental conscious contents gathered together in 'bundles' through the agency of association, the investigation of mind has been turned to concrete common-sense experience as the only truly empirical point of departure. Instead of setting out either with a preconception of machine-parts and the ideals of an assembling-plant, or with the vague though much more human notion that all that is works together for the good of man; the attempt is at last being made to look experience in the face, and to observe what it is really like. In taking this departure we find ourselves in the way of discovering some very interesting things—as the numerous and exciting reports upon the phenomena of perception have already shown. But we do not find 'sensations', 'images', or 'affections', or even 'imageless thoughts', either singly or in combinations. We do, indeed, find sensory aspects of experience, and others which we may call imaginal, affective, and cognitive; but they do not fall into slots or specially prepared bins from which they can be taken and put together again to reproduce the experiences from which they were derived.

Neither are they *intentional* in the sense in which a universal purposiveness or teleology would suggest. On the contrary, these phenomena are as concrete and as capable of accurate description, as those one observes in a test-tube. They are not shot through with impending consequences, nor do they hark back to antecedents. Instead, they are immediate and definite phenomena set off in curious ways from the vaguer ground of consciousness upon which they are cast, and they possess an unique construction which is partly immanent or internal to themselves and partly dependent upon their setting as indicated by the nature of their 'contours' or margins.

Now the analysis of such a perceptual configuration is carried out in terms of its members, and a complex whole must, of course, be reduced to a smaller whole before its organization

[2] *Cf.* K. Koffka, *Die Grundlagen der psychischen Entwicklung*, 1921, 73 ff.

can be fully comprehended. Here we revert to the same means of control that we have always employed in experimental psychology. Ultimately, I believe, we must study the nature of all these configurations in terms of the same attributive aspects we formerly applied in our search for elements, adding, however, such other attributes as may come to light in the course of our investigations. Yet our search is no longer for elements, but for the underlying integration of attributes which makes compulsory the apprehension of concrete phenomenal wholes: things of various sorts, and movements of various sorts—and thoughts of various sorts.

When, therefore, I view some of my earlier experiments upon the consciousness of 'meaning',[3] undertaken while 'the Würzburg School' still flourished, I am struck, first of all, by a certain remoteness of the point of view I then held. But in rereading the reports of two rather extraordinary undergraduate observers, I am also impressed by the vivid freshness of many of the data they recorded in describing the meaning of a word. Indeed, I find in these reports certain *qualities* which, to me at least, possess evidential value as true descriptions of the phenomena of 'meaning,' for they strike me as far more realistic than any mere assemblage of elemental bits of mind-stuff ever could be. While the inferences then drawn from these data seem to me no longer tenable in their precise intention, I still find such phenomenological essentials as even a perverse elementalism could not entirely destroy, and it is upon these that I think it now possible to construct a more adequate interpretation.

The experiments to which I have reference consisted in series of words presented to the observer singly with the instruction to secure the word's meaning and to react in a predetermined manner as soon thereafter as possible. A report was called for, and the data obtained were later analyzed. The net results of these analyses were then summarized in a series of statements[4] to the effect that the meaning of a word is given as a *mental act of relating* in which the terms of the relation may be the stimulus-word and a second content, or the terms may both be contents other than the stimulus-word. What appeared to control the whole procedure was referred to as a 'dispositional matrix' called the *nucleus*. This nucleus, formed under the joint influence of the instruction and the stimulus-word was found to determine the range of association which the word might have. The contents emerging from the experience were therefore regarded as associates of the word, or of some aspect of the instruction, or of the surrounding circumstances. Since

[3] *Op. cit.*
[4] *Op. cit.*, 116 ff.

the 'mental act of relating' which gives meaning was assumed to be selective, the relevancy or irrelevancy of the emerging contents was described as being *felt*. A distinction was then drawn between felt relevancy or irrelevancy and a tendency towards *expression*, *illustration* and *example*, leading to the appearance of other associative contents, usually imaginal or verbal, which, by virtue of other relations established with the nuclear meaning, gave an illustrative precision to the thought. These illustrative expressions, however, were not found to be essential to the meaning, and were often felt by the observer to be inadequate to it. Yet being, as a rule, obvious, definite and concrete, they often seemed to make out the complete content of the experience. Nevertheless, the meaning-process could be readily distinguished by the observer from these supplemental processes of expression.

In my own defence I may be allowed to remark that my conclusions were expressly labeled 'tentative', for what strikes one immediately is the indescribable *act of relation* with its selective function. Otherwise the description would perhaps even yet pass muster. My present purpose, however, is to review it briefly from a more recent, and, it seems to me, a more significant point of view.

What does the observer have before him in an experiment of this kind? First of all, an instruction which somehow, as we should probably all admit, paves the way for his response. Let us call this a dispositional readiness for the appearance of certain mental phenomena. The first phenomenon of the experiment itself is the stimulus-word; this is perceived, and in its perception varied experiences emerge, simultaneously and in train.

If we proceed to analyze these phenomena from the point of view of elementalism we must look for entities, and they must appear in associative constellations or chains. But if we renounce elementalism and associationism along with it, then our phenomenal data take on quite a different aspect.

We said that the stimulus-word is perceived; to the older view this only meant that it was *sensed* or *felt*; the interpretation came afterwards, when, through association, it had revived some other content or contents than itself. According to the newer view, the word itself, as sensed, is at once the *nucleus* of the experience, the dispositional matrix being its neurological counterpart. Phenomenally, that is psychologically, the 'felt' word is already its meaning; for *there are no meaningless experiences*.

The error of previous interpretations lies, I believe, in neglecting the stimulus-word. As I reread the reports of my observers I find references again and again to pronunciation of the word, when it was presented orally, and to the 'looks of the

word', when it was presented visually; even the association, as I then called it, of the word's sound or appearance with its later more completely expressed meaning was repeatedly signalized by the observer. What is a word, indeed, but a member of a complex situation in which it is employed? Without employment it has no existence, but once heard or seen, or otherwise apprehended, it is and must be accepted as part of a phenomenal whole.

The relation of the stimulus-word to a subsequent datum now seems to me a mistaken inference; for there really are no separable data in these experiments. The instruction which determines the configuration of the experienced phenomenon holds these focused upon the perceived word until the experiment is over. To be sure, the mind may wander from its task, and new centres may be focused in new perceptions, but so long as the instruction controls, the meaning is phenomenally the perception of the word: and all else is logic rather than psychology.

The relation and its terms, phenomenally considered, comprise a subject too large to be treated in this paper, but I should like to suggest two conceptions which I believe will be helpful in elucidating this problem. The first is the conception of the crude figure of any experience as it emerges in consciousness; this, I believe, is what my observers experienced in the first moments after the word was presented to them. This figure will be more or less crude according to a variety of circumstances—the ability of the observer, his previous experience with this word, his readiness for its reception, etc. All these condition the 'feel' of the 'nucleus.' But something more is necessary before the word is phenomenally defined; for the conscious figure of the nucleus must be bounded. With respect to the figure as centred or focused no logical relations can be established, because no datum is given to supply a second term. But let the figure be bounded in any way, and relations are at once possible with respect to various aspects of its boundary. Thus the meaning of a word *just so* is phenomenally the pattern of the whole experience that is given in the initial perception of the word. To this there accrue in short order details of integrative pattern, themselves partial integrations, which contrast with and set off one member from another. The nature of these detailed members is in part reproductive or imaginal, as we have been wont to say, but partial integrations are likewise manifest which, when later expanded in the observer's report, appear as if they were self-sufficient data or contents. Certain of these may lack all semblance to any sensory quality. In principle at least, partial integrations of *intensity*, *extensity*, and *protensity* are quite as possible as the integration of *quality* and *protensity* which seems to account for the pattern of seen-

movement. In this sense 'imageless', or non-qualitative, 'thoughts' may be experienced as dependent members of a larger phenomenal construction, which nevertheless are capable of a certain isolation and of an accurate description in attributive terms. So far as I know, this isolation of non-qualitative phenomenal members has not yet been attempted, under exactly controlled experimental conditions, but I see no reason to question that it may be done.

It is, however, the definition of contours in the total moving pattern of thought that plays the greater part in the development of meaning, since it provides the basis for a description of the partial integrations to which we have referred. Without observing the margins of a figure, its structure remains incomplete and indefinite, and while the vague 'feel' of any integrated unit is already a meaning, its meaningfulness increases, that is, it can be subjected to a more refined logical treatment, only after its pattern is clarified by the observation of its margins or contours.

Rubin in his study of visually perceived figures[5] remarks that we know as yet little or nothing of the psychological nature of the 'contour,' but he goes on to show in a variety of interesting experiments that the internal structure of a perceived figure is not dependent upon a definite contour even though some 'margin' or 'edge' to the structure must invariably be given. So it is, I believe, with the meanings of words seen, heard, and articulated; they too are set in a complex matrix of 'feeling'—not primarily affective but sentient; and this 'feeling' constitutes the phenomenal character of the thought, its configuration being, in principle, always capable of reduction to attributive terms. Thus, though meanings are often ambiguous and complex, they have a quite simple origin in the perception of a systematic unit or whole which is conditioned by physical and biological data equally systematic in nature.

Which brings me to a final word upon the philosophy of this interpretation. Instead of being compelled to humanize or idealize the foundations of psychology and to assume *intentionalism* as the guiding principle of the unity of mind, we find here a phenomenal unity describable in terms of its own attributive characteristics without reference either to utility or to an *ego*. On the other hand the position taken is equally remote from that of supposing a *mechanism* which necessarily deals in entities either sensory or neural, fitted together by some unknown agency—mere chance, perhaps—acting and reacting upon each other as particles conditioned by blind and inscrutable forces.

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[5] E. Rubin, *Visuell wahrgenommene Figuren*, 1921, 106.

Instead, we have the conception of a system inherent in the data themselves, not somehow added either by arbitrary juxtaposition or by the Author of the Universe. And though for convenience it may seem desirable to hold apart the system of physics from the correlative system of conscious phenomena, and perhaps likewise an intervening system of biology, the inference is clear that these are but different ways of approaching the nature of one reality; and that although phenomena are observed and defined in different ways and by different means, according as we call ourselves physicists, chemists, physiologists, zoologists, or psychologists, the correlations we are able to establish between these fields are justified by the fact that one meaning underlies them all; mathematics and logic being but the tools man has devised to approximate this meaning in his attempt to describe its pattern.

THE CONSCIOUSNESS OF RELATIONS

By F. Russell Bichowsky, University of California

Several modern investigations of the conscious content have agreed that the consciousness of relatedness, whatever else it may be, is not an "elementary feeling" in the sense that the consciousness of blue or other sensations is an elementary feeling. Thus, Gleason[1] finds: "In all of our experiments with nine observers, a relational element was not once observed. The existential processes are the usual imagery, the kinaesthesis attending bodily movement, and organic pressures and warmth."

Gleason's conclusions may be criticized on the grounds that in the set of experiments she most emphasizes, namely those employing as stimuli pairs of words such as "nebula-solar system", the relation, the consciousness of which it was desired to investigate, was not present in fact between the stimuli or between the resultant images or "ideas," but must be invented by the observer. My experiments have shown that this invention is, in reality, a complicated psychological process. Thus:

No. 41 Observer B. Non-visual type.
Instructions: "Two objects will be named which commonly stand in familiar relation to one another. When you are aware of what this relation is you will indicate it by saying yes. You will be asked afterwards for a full report of your conscious process."
Spoken stimulus: "cat—bird."
Report: "Fore-period. Felt relaxed. Attention wandering. Vague kinaesthesis as of movements in the skull. I did not expect the stimulus just then, and on hearing it I sort of moved forward and drew up in the chair. This was on hearing the word *cat*. I don't remember any special effect of the word *bird*. My attention dropped and I had vague auditory and verbal kinaesthetic images of the lecture (a preceding lecture on zoology). Verbal image 'feline' stood out. The next thing I remember was feeling my strained position. Verbal image 'bird'? as if said with a rising inflection. Kinaesthesis in forehead meaning, I suppose, 'What family do birds belong to?' Verbal image 'avis.' The next I remember was 'coelenterate vertebrate.' These were strong verbal images. Then a feeling of comfortable warmth. Said 'Both vertebrates'." Time 3.2 sec.

Moreover, Gleason's instructions and my earlier ones (for example in the illustration above) may be criticized on the grounds that they call for a report of "relations". Gleason herself remarks on the difficulty her observers had in deciding what was a relation and what was not a relation between her

[1] J. Gleason, this Journal, 30, 1919, 1ff.

stimuli, whether verbal or graphic; thus, one of her observers reports "two parallel lines" and yet is unable to say what is the relation between them. I also early found that very few of my observers had a clear idea of what a relation was. In fact, regardless of the fundamental part taken by relations in logic and philosophy,[2] the abstract idea of relations is very difficult. Still, this lack of familiarity with the idea of relatedness in the abstract may itself be taken as an indication that the feeling of relatedness is not as prominent a part of our consciouness as the 'feeling' of color for instance. Moreover, considering the excellent quality of the reports obtained by Gleason, it is hardly likely that, if there had been present any 'elementary feeling' of relatedness in general, even as vague as the kinaesthesis involved in the impulse to do some external act habitually reported by her observers, this elementary feeling would have escaped report, even if not recognized as that of relatedness. However, if the feeling of relatedness were as vague an impression as the kinaesthesis of intercerebral movement which occurs with certain observers in all difficult thought-transitions, or even if it were as vague as the imagery-kinaesthetic complexes into which Clark[3] analyses "mental attitudes," it is quite possible, as experiments with misleading instructions show, that it might be overlooked simply because the observer was not sure for what he was to look.

In my own experiments[4] I used for stimuli at various times:

1. Pictures of two objects in some relation: *e.g.*, a book on a table; a boy climbing a tree. (This is Gleason's method No. 1.)
2. The names of two simply related objects: *e.g.*, Baltimore-Washington; father-son. (Gleason's method No. 2.)
3. The rule of three: *e. g.*, red is to pink as violet is to lavender. (Woodworth[5].) This method was used both with and without the last term given.
4. Simple relatives: *e.g.*, . . is greater than . . ; . . . . implies . . . .
5. Partial sentences: *e.g.*, The telephone is standing on the . . . . My . . . . is my mother's sister.
6. Propositions in symbolic logic and mathematics: *e. g.*, $p \supset pvp$.

My instructions in almost all cases called for a reaction in the form of an appropriate sentence; *e.g.*, a judgment involving the relation. I did this, as I felt it needless to complicate the thought-processes of the observers by requiring them to report the relation *ex situ*, which is logically speaking not a relation but a term, and which to introspective analysis will be shown to be certainly different from the psychological correlate of the relations sought for.

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[2] F. R. Bichowsky, *J. of Phil.*, 14, 1919, 518; 17, 1921, 295.

[3] This JOURNAL, 22, 1911, 215.

[4] These experiments were begun in 1911 and for the major part were completed in 1916.

[5] R. S. Woodworth, in *Essays in Honor of Wm. James*, 1908, 285.

My methods, which were the usual ones, need not be described in detail here. I had no difficulty in getting very complete analyses. Yet even in the case of observers M and myself, with whom at first a strong prejudice existed favoring the discovery of a weak "feeling of relatedness", no such "feeling" was clearly observed, and in cases where it might be inferred or indeed was vaguely reported further analysis has shown that the results may be interpreted in a different manner.

V 14 Thus for observer V we find the fourth term of the proportion "red is to pink as violet is to....." reported as "lavender" immediately, 0.5 sec., without remembered intermediate processes. Yet the observer must go through an elaborate imagery: *e.g.*, Titchener color spindle, kinaesthetic placing of all four terms on the spindle, the discovery that lavender and pink are close together, that they are tints, in order to formulate the relation "pure colors to tints."

Logically it would seem that some feeling corresponding to relatedness in general is necessary to explain the right response, since the knowledge of what the relation was only came late in the process. Psychologically, however, this conclusion is by no means necessary, since many cases are known where a specific reaction occurs before it is possible to recognize the class, or name, under which the reaction falls. Moreover, repeated analyses of cases which differ from the above only in the fact that the first phase could be analyzed have failed to find any elementary feeling of relatedness in general.

If, therefore, there is an elementary feeling of relatedness it must be of extraordinary lack of clarity, since it would seem hardly likely, in view of the wide variety of methods used by Clark, Gleason and myself, that any experimental condition necessary to observe a vivid feeling of relatedness could have been overlooked[6].

An even more definite discussion can be made as to whether there is any complex of known elements, *e.g.*, any kinaesthetic or other imagery, to be found in all cases of relational experience.

My observers, like those of Clark, were able to report repeatedly imagery complexes corresponding to such "conscious attitudes" as "surprise". They were able also, in some cases at least, to report what may be taken as the imaginal complex for a particular relation; for instance, the relation "jumping" was for one observer (K No. 21) often accompanied by the kinaesthetic sensation as if the observer was flying through the air, even where the resulting conscious pattern was grotesque; *e.g.*, where the stimulus was the "cow jumped over the moon". Still, my observers could find no complex corresponding to relation in general, that is, no common "conscious attitude" in all cases of consciousness of relations, though it is difficult to see how it could have been overlooked if it existed.

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[6]My *O*s were not aware of the results of Clark and Gleason or of the work of the Cornell school in general.

Of what, then, does "relation consciousness" consist? If it is not sensation, elementary feeling, or imagery complex, it is obviously not to be found by methods of introspection which consider as significant only reports of sensations, elementary feelings and complexes of these elements. By rough analogy, the problem of describing conscious relations is similar to the logical problem of describing any objective related nexus. Logically, it is possible to attempt this by merely listing the elementary things of which the nexus is composed. Such an analytical description gives all that has objective being in the nexus, but misses entirely the relations. So classical introspection, which is after all a process of objectifying[7], is unable to discover the transitive non-substantial part of the conscious nexus. The only way in which relations appear in an introspective report is when they crop out in the form of some statement of meaning[8], interpretation or reference; *e.g.*, in such judgments as "The lines were parallel"; "This sensation inhibited the other"; "An image of the door".

Negatively, such relations are any part of the report of a conscious content not a mere statement of sensations or elementary feelings or a statement that can be analyzed completely into a complex of these units. Empirically, relations and other meanings appear quite distinct from the sensation, feeling, and imagery content of consciousness and seem to belong to a separate realm of psychic entities. Jacobson[9] has been one of the first to include such statements of meaning in the body of his introspective reports, and though I am inclined with Titchener[10] to think that it is difficult to find a sharp line of division, I shall adopt the practice.

As a matter of fact, my experience both as experimenter and as observer has shown that, while it is difficult at first for an observer who has been trained to attend only to the sensation

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[7]R. Dodge, this JOURNAL, 23, 1912, 214; E. B. Titchener, *ibid.*, 23, 1912, 165, 427, 485.

[8]"Meaning": E. Jacobson, this JOURNAL, 22, 1911, 553. "Kundgabe": E. von Aster, *Z. f Psych.*, 49, 1908, 69. "Sprachlicher Ausdruck:" E. Dürr, *ibid.*, 49, 1908, 315.

[9]Jacobson, *op., cit.*

[10]E. B. Titchener, this JOURNAL, 23, 1912, 165. I am not clear whether Titchener means to depreciate the report of meanings in introspective accounts of conscious content on the ground that such reports meet the logical demands for an interconnected account rather than the psychological demands of the situation. In any event the requirement that psychology be a purely analytical science, *i.e.*, report only the units of which its phenomena are composed and not their interconnections, even though these are of a different type, appears to me as arbitrary as to require chemistry to report only what chemical elements compose its substances and omit an account of the laws that control their combination.

content of consciousness to learn to report meanings, this is largely due to the setting up of inhibitions. These inhibitions can be overcome, however; and with untrained observers I have found, if anything, less difficulty in teaching them to report meaning and sensation content ("the processes"[11] in Jacobson's terminology) than to report sensation content alone. Indeed, I think all experimenters will agree that, even with the most highly sophisticated observer, it is difficult to get a pure "process" report; the instinct of the observer is towards reference of sensation.

I did not find, however, that exact correlation of meaning with "process" that Jacobson seems to imply. The situation is somewhat more complicated than the mere existence of "two parallel accounts, in terms of process and in terms of meaning, of one and the same total experience." In the first place, I found that "meaning" was a rather vague word to describe that part of an account of consciousness which could not be considered a dscription of "process".

The parenthetical passages as they appear in Jacobson's reports are, in the majority of cases, mere cases of reference of sensation or image to an outer object; *e.g.*, "White (from the paper)", *op. cit.*, 557; "Visual image (of an apple)", 559; but there are other cases, especially where the parenthetical passage is a sentence, where we have to do not with direct reference to an outer world but with some sort of interconnection among feelings and sensations themselves; thus: "then mixed-up feeling, unpleasantness, nausea (all this meaning, I don't know what to do)."

In classifying my results, I have found that my *O*s report as meanings all the types of references given above, and also relations among sensations, feelings, images, and other meanings: *e. g.*, such relations as "this image seemed larger than that", "the image of the house sort of flowed out of that 'idea'." I was more particularly interested in this restricted class of meanings, namely, known relations; and my discussion will be restricted to this special case, though many of the facts and conclusions drawn can, I think, be immediately applied to "meanings" in general.

Typically, knowledge of relation between A and B appeared in connection with a change from image A to image B. Just when the knowledge of the relation appeared in the time-series, it is difficult to determine. Apparently, the "knowledge" did not always appear at the same place. It often occurred in the series: (image: A) (knowledge that A R B) (image: B) in the order given, but sometimes last, and rarely first.

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[11] "Process" has always seemed to me to be a most unhappy term to describe the sensation, image, feeling content of consciousness. One of the most characteristic features of these so-called "processes" is their possession of the quality of being a psychic stuff or substance, and of corresponding to, or seeming to be, the *thing* outside. I should be tempted therefore, to call them "substances", not "processes". To be sure, they are subject to change; but so are physical substances.

Thus C (visual type), a student of symbolic logic, reports on his mental process of discovering whether the relation of implication occurs between random logical propositions. "In all cases where I recognized implication immediately the image of B seemed to flow out of that of A often before I was even aware of seeing B". Again: "There seemed to be a kinaesthetic focusing on image B, though the awareness of implication did not seem to be the focusing or either image."

This is the simplest form. In other cases, more complicated steps were gone through. Thus V (visual) reports after the auditory stimulus "John jumped the creek"[12]: "I had an image (of a man in a track suit hurdling). The image (of the creek in the park) flashing up blotting out that (of the track meet). Then the image (of the man in the track suit) gradually reappeared (as jumping the park creek). (The thing was quite incongruous.)"

Very often, the transition connected with the meaning was not between images of the things (which were judged to have the relation), but between some extraneous images coming from the preliminary set, or between some modification of the images produced by the preliminary set[13].

Observer B was asked to report in the form of a sentence as fast as possible the relation between two figures to be shown. Foreperiod report: "I felt strain. Kinaesthetics of setting myself as at the start of a sprint". Stimulus: a man sitting on a standing horse. Main report: "I didn't get the meaning at first; then the image of a horse racing came to me; then I noted the old man on his back as in the picture and reported: 'The man is racing the horse'.....No. That didn't seem incongruous. Was it?"

In certain cases an actual inversion of the relation took place.

Observer B. Direction: "Give first significant sentence of the form A has some relation to B' that comes into your mind, on hearing the two words I will give you. Give an introspective report of the total thought content."

Fore-period. Report: relief of strain. Kinaesthesis of relaxing into the chair. Meaning forgotten. There seems to have been one, probably "That's easy." Wandering attention; *i.e.*, vague articulatory kinaesthesis, ending as if to say 'relation.' Sensation of warmth.

Stimulus: pot, 0.5 sec., lid. Main report: "Start; kinaesthesis of inhibited breathing; whispered "pot" and sort of automatically "is..... on stove;" I was groping. Strain present. Jerk as of pulling up in chair, associated with the meaning "That's wrong?" The question-mark seems to belong there. Nausea: articulatory kinaesthetics of saying 'pot is on?'....'on?'....No associated meaning. Then automatically I said to myself, 'lid.' There was pleasing sensation, and feeling of relaxations. "Thats it," I thought. Reaction: 'pot is on the lid.'" Time 2.4 sec.

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[12]In this report I have distinguished the "meanings" in accordance with Jacobson's convention by parenthesis. If one wishes to see the barrenness of pure "process" reports he should read the above report omitting the parenthetical passages.

[13]May, *Archives of Psych.*, 39, 1917; R. H. Wheeler and T. D. Cutsforth, this JOURNAL, 33, 1922, 361.

However, the most striking evidence for the transitive quality of known relations is the fact that my observers all recognized the transitiveness without previous suggestion. Various terms were used to express this. The images involved were said to have "led to", "carried over", "flowed into" the meaning. And *vice versa*, the meaning "was felt relative to", "caused", "was transitive to", "flowed into" the image. Sometimes the image was said to carry the meaning; but all our observers denied that the knowledge of a relation R (that is, the meaningful idea "A has the relation R to B") was the same as an image of A in the relation R to B. Thus the knowledge of the relation in the sentence "The cat is on the table" was carefully distinguished from the image "The cat on table," though this knowledge was associated with some transition of images often, though not always, involving this image in the chain. For one observer as a rule under certain types of stimulus the knowledge that the cat was on the table rarely involved the image of the cat on the table, but separate images of cat and then of table. This was almost invariably the case when the relation was not spatial.

In our best reports, the correspondence of known relations and transitions in sensation-image-content was so complete that it occurred to me that it might form a criterion for grading introspective accounts. I therefore, at periods of approximately two months, gave two of my *O*s the same material for introspection. The results for one *O* are given in Fig. 1. Crosses represent the number of relations reported, circles the

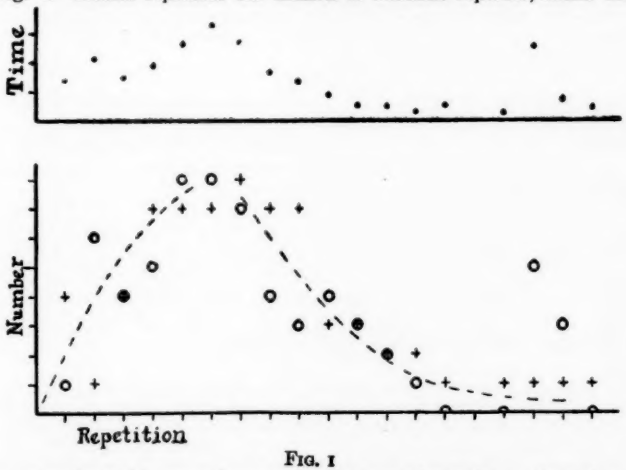

FIG. 1

Practice habit curve for repeated introspections; above: time of reaction; below: number of relations reported (+), number of images, etc. (o).

number of feelings and undifferentiated images, and images. The time of reaction is plotted in the supplementary figure. The *O* was practically untrained at the beginning of the test. There is shown here a period of growth of ability. The total number of known relations and "processes" shows a gradual increase, as shown by the dotted curve of average value; but the distribution of this number into two groups, relations and images, etc., is erratic, and seems to be due to varying focus of attention. At the maximum and during the remaining portion of the curve with the exception of two points, that is, during the period where the reaction was becoming habitual and not conscious, correlation of relation and images, etc., is very close. If anything, apparently the tendency is to lose images from consciousness before meanings. The exceptional two points near the end represent a case where special effort was put forth by the *O* to inhibit the effect of habit, slow up the reaction, and treat it as a new experiment. These results are included only because they show that it is possible in a certain measure to dissociate the effect of habit from an apparently well established habitual reaction. The correlation of time and number curve requires no comment.

The experiments with the second *O*, though not so complete, show the same general characteristics. The curve for decay shows the same form as would be expected from the general law of habit formation. However, the curve for learning shows a wide variation. The actual curve of ability in introspection may be considered as a resultant of a curve for learning (which shows much individual variation) and a curve for habitual decay (which curve is less subject to individual peculiarities). Perfect introspections, giving all the meanings and images that actually occur in a novel thought-process, could only be obtained on first trial, as after that habit-formation begins to blot out features of the original process. On the other hand, even with highly trained *O*s, the difficulty of attending and remembering is so great that practically many of the significant features of a novel thought-process are not noted or remembered on the first experiment with a new stimulus. It is plain, however, that reports which are to be used to correlate images, etc., with meanings should be taken from the maximum portion of the curve, that is, after several repetitions (at long intervals) of the stimulus.

Using such reports, I found not only that known relations as they appear in meanings show a transitive quality, *i.e.*, are always connected with some transition in the content-account, but also that they show other differences from images, sensations and feelings which are significant.

(1) The known relation has a sort of pseudo-independence from the images, sensations, etc., which it serves to connect, in that while it never occurs except in connection with some terms it is apparently independent of any particular term; for the same relation can hold between a wide variety of images, two relations may be identical, different, etc.

The exact nature of this pseudo-independence of known relations and terms, as well as the "universal" quality of known relations, which we are about to describe, can be most simply described by pointing out an analogy in the field of logic, namely, the pseudo-independence of logical relations and terms. A relation in logic is not an entity possessing separate existence; for by definition such an entity would be a term; yet it must be pseudo-ndependent of its terms in the sense that the same relation has signifi-

cance when holding between different pairs of terms. Thus "A and B" represent the same relation as "C and B", namely, being taken together. Another way of saying the same thing is to say that, though relations are not terms, they must be capable of having a name. These results are accomplished in logic by treating any particular relation as a universal. Thus, a particular relation is defined by its most general connotation; it is that relation holding alike for all pairs of terms that in fact can stand in that relation. 'Jumping over' is thus the relation which holds between all pairs of terms such as the cat and the fence, the cow and the moon, which can conceivably stand in the relation jumper-jumped. Using a less technical analogy, a known relation may be conceived as a "direction" by which we go from image A to image B. "One hundred yards north-west" has no significance except from some particular point, yet it has significance when applied to almost any point on the earth's surface.

Thus our observers (who were not trained logicians) had difficulty in getting what *to them* were satisfactory accounts of the phenomena of knowing a relation. One remarked, after a long attempt to improve his introspective reports; "I don't know how it is, but the relation never seems to come without some sort of image, yet it seems to be independent of any particular image.....I can't get it under attention. It just flashes out."

Titchener,[14] reexamining some of Jacobson's *Os*, asked them to give an account of the introspective differences between meanings and processes (our sensation, feeling, image content) and obtained a series of introspections which may well become classic. Both of his *Os* agree to the pseudo-independence and immediacy of consciousness of meaning, though they do not observe its transitive relation to images. I suspect this is in part due to the unfortunate connotation the word "processes" has. Thus D: "A mental complex....as it shows itself to introspection, rises, becomes more or less prominent, dies away.....meanings are (in this sense) not transitory, but are perfectly definite and discrete things, distinct from one another." F: "I have never noticed, however, that meanings overlap or pass into one another; the meaning is, I think, always this or that at any particular moment."

D: "In particular (*a*) meanings are definite and arrange themselves easily in sequences, whereas it is difficult to be sure of the temporal order of processes.....(*c*) Meanings go naturally into words; the phrasing taking care of itself. One has to be very careful of one's vocabulary in reporting processes....The statement of meaning is altogether independent and can be made without any thought of processes. I find, however, that even when I am trying to hold strictly to the attitude of reporting processes, I am tempted to refer to meanings in order to make the description complete and chronological. Where the statement of meaning (as in some of the new experiments) is given (reported) first, it offers itself as a skeleton on which I may reconstruct the process-account; it promises to give shape and organization to that account; and I cannot help making use of it. While therefore the introspective attitude is *analytical*, the meaning attitude is not analytical at all, but just a matter of course; the expression takes place immediately without hesitation as if automatically."

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[14]E. B. Titchener, this JOURNAL, 23, 1912, 180.

F: "The meaning stands as a unit which cannot be analyzed; it stands individual and irreducible... This "belonging together" on the side of consciousness and "single direction towards an end" on the side of meaning .....Processes move. They begin and grow and end. They have attributes; meanings are static,...they merely are. They do not possess attributes."

Another line of evidence pointing to the pseudo-independence of known relations is afforded by the failure of a series of experiments where the original purpose was to attempt to identify the knowledge of a particular relation between two things with the image of those two things in that relation. I attempted this by giving double stimuli, *e.g.* the sentence "The cat is on the table," and then, almost immediately, 0.1 to 1.0 sec. later, the picture of a cat on the table. With the comparatively untrained observers used (this particular experiment is best carried out with comparatively untrained observers), the occurrence of the first stimulus would often be completely forgotten. Yet the reaction was almost invariably a reproduction of the sentence used as the verbal stimulus. It was thought that since in these cases, as far as the observer knew, the reaction proceeded immediately from the visual stimulus, he would have no "Aufgabe" reason for distinguishing related images from the knowledge of the relation. Yet, typically, our comparatively untrained observers refused to confuse the recognition of a relation with the images in which it held. "I know the relation, I don't see it," one insisted. Again, "No, the relation wasn't the images. I just knew it."

Direct introspection of the mechanism of recognition is always difficult, and introspection on the recognition of relations (the author speaks feelingly) is more difficult than usual.

The problem set was the recognition of a given relation between various pairs of objects, some of which did and some of which did not exhibit the relation in question; *e.g.*, for the relation 'square' the pairs of numbers 1, 1; 5, 2; 4, 2; 2, 4; etc, were given. If the relation was familiar, our *O*s, of course, had no difficulty in recognizing whether the pair exhibited the given relation or not. They must therefore in some manner carry over the idea of the relation given from the instruction period to the main period. Yet even in these cases the *O*s were emphatic in refusing to identify the meaning of the form of words often used to "carry" the meaning from fore to main period with the relation. Often the words which were to give the *O* the knowledge of the relation. *e.g.*, "is square of" were merely repeated as nonsense phrases to which meaning attached itself only when definite terms were presented. "Thus, when 3 and 4 were presented I knew immediately that 3 was not the square of 4. I recognized that 4 was the square of 2 as soon as these were presented."

More experienced observers even had difficulty here, but the results obtained are in harmony with the observations of Titchener's observers that recognition of a particular relation

is immediate, in the sense that there is nothing in the consciousness of a particular relation which corresponds to the several processes involved in recognizing a conscious image as that, say, of an apple. This, of course, qualitatively, is a consequence of the fact that no new sensation elements corresponding to relatedness are to be detected in cases of consciousness that a given relation holds. However, it also appears, from what little experimental evidence we had on the actual time of recognition, that in some cases the presence of a relation between two objects actually cut down the time necessary to recognize the two objects, not presented as related.

As far as the time necessary to recognize words *versus* sentences or letters *versus* sentences is concerned, this result has been established by several other experimenters. In our cases, where the stimuli were objects presented in familar relations, the results are not so certain, but appear to be significant at least of the short time necessary to recognize relations *in situ* compared with the relatively long time necessary to go through the steps sensation, image, meaning. If the decrease of time for the recognition of A in relation R to B, in comparison with the time for the recognition of A and B separately is proved for any large number of simply related pairs, this will show not only the absence of any new physiological or psychological steps in the recognition of 144 as the square of 12, as contrasted with recognizing 144 and 12 separately, but also the actual existence in such separable perceptions of some time-consuming process (inhibitions, most likely) not present in judgment.

Our best trained observers added to the analysis of meanings given in the reports of Titchener's observers the observation that the particular relation always had the quality of a universal.

Thus: "When I try to catch the relation (north-westness) as apart from the particular places given, or try to find what I mean by north-westness, that is, when there is a question as to whether the relation between the two places given is north-west, or when I focus on north-westness, I always get an image not of any two particular places one of which is north-west of the other, but of a map of U. S. On this map I locate the places, then note their relative position. It is during this process that I become aware that one is north-west of the other". Again "North-west in the abstract, as far as it suggests an image to me, suggests a series of parallel lines drawn on a map of the United States." Another *O* (B) reports that consciousness that A is north-west of B "comes during the placing of A and B on a vector field with arrows pointing to the upper right hand corner." Another, V, remarks: "I seem to be standing in Washington (one of the places given), looking up 16th St. Baltimore is in front and to the right. It was during the development of this image that I became aware that Baltimore is north-west of Washington," Other *O*s all reported this last fact, namely, that it was during the development of the image that recognition "meant" a type of relation come. This may be taken as suggesting that the meaning of images, like the consciousness of relations,

is relational in character. The image, however, is not identified with the relation which it is said to "mean", "carry", "suggest" or illustrate. In this respect the image which carries relational meaning is quantitatively, though perhaps not qualitatively, different from the sensed image of a thing which, at least to the same order of introspective *finesse*, is considered to be the thing as perceived. Thus observers will report seeing an apple, meaning thereby having the apple-image of a particular, but will not report seeing the relation north-west or having the image of a particular north-westness. What they do report is: "I have the image which, when particular points are supplied, is associated with my knowing the direction between those points. Until the points are given it is hard to focus and doesn't mean anything clearly."

Our observer trained in symbolic logic recognized the situation right away. "Why", he said "the particular relation has to me always the quality of a universal. It is carried by an image in the same way that goodness, all men, geometrical triangles are carried.....When I talk in logic about all men being mortal I don't have an image of a row of men as B (one of the observers) says he does, but of a particular man. Just now it is of a tawny man, the particular image probably being suggested by the remark of Berkeley on this subject which was in my mind. The image is always of one man, not necessarily always the same one, though. The image isn't identified with the idea of men in general. It simply acts as a peg on which to hang it. So with the idea of a particular relation abstracted from any special term, as when I recognize the relation. It does seem rather strange to talk of a particular relation like north-west as a universal, doesn't it?"

This suggestion of our observer seems to give us both the most reasonable terminology in which to put our facts and also, by suggesting the identity in type between images carrying "universals" and those carrying relations, to simplify greatly psychological description. Thus, it accounts for the puzzling fact that in a large proportion of cases no images other than fugitive and incidental images connected with the fore-period[15] rather than with the stimulus used occurred, but rather a definition with the images appropriate to it.

Thus C once reported as the image for the relation of implication (indicated by Whitehead and Russell by the symbol ⊃) the image—AvB, which is Whitehead and Russell's definition of that symbol. So B: "The word 'the square of' at first had no meaning.....Then came the meaning X times X, with the image X×X. The images were very difficult to recall; the meanings were, however, very vivid."

The hypothesis also accounts for the somewhat complicated introspective phenomena reported in experiments involving

[15]These may be identified by means of the accompanying meanings This is one of the advantages of reporting meanings along with images.

transposition of relations from one set of images to another, which cannot be reported in full here.

Making this hypothesis, the results of this paper may be now summed up very simply. *Consciousness of two terms in relation involves the existence of a relation between two psychic entities (images, sensations, etc.). The psychic, and presumably the physiological, correlate of the known relation is a relation.* There is no new psychic entity corresponding to relation, in the sense that there is a new psychic stuff (the image, sensation, etc.) corresponding to a thing. This position is perhaps what would be expected from our ordinary observations on relations as logical entities, but psychologically it is unexpected or at least was unexpected by the author; for by analogy it was thought that knowledge of relations would be *via* image in the same way as knowledge of things. It, however, produces some simplification in our treatment of psychic content.

THE EFFECT OF INTENSITY OF ILLUMINATION ON THE ACUITY OF THE NORMAL EYE AND EYES SLIGHTLY DEFECTIVE AS TO REFRACTION

By C. E. Ferree and Gertrude Rand, Bryn Mawr College

Considered with reference to the eye there are three factors in acuity or the power of the eye to see clearly,—the resolving power of the refracting media, the space discrimination of the retina, and the sensitivity of the retina to light. To put it in another way, there are the resolving power of the refracting media or the power to form clear images on the retina, and the resolving power of the retina or the power to discriminate detail in the physical image formed. The explanation of the effect of increase of intensity of light on the power to see clearly is to be found, of course, in terms of the resolving power of the retina, not of the refracting media. The purpose of this study has been in part to determine the effect of increase of intensity of illumination on the acuity of the normal eye and to compare this effect with that obtained with the same eyes made slightly defective as to refraction.

The Effect of Intensity of Light on the Acuity of the Normal Eye and Eyes Slightly Defective as to Refraction

In the series of experiments on the normal eye four observers were used, and the acuity was determined at 0.001, 0.005, 0.01, 0.015, 0.02, 0.05, 0.1, 0.2, 0.4, 1, 2, 3, 5, 10, and 20 foot-candles of light normal to the test object. The eye was adapted to each illumination by a 30 min. practice series with proper rest periods. The broken circle (the international test object) mounted on a rotating graduated dial was used as test object. In making the observation all that was required of the observer was to indicate the direction in which the opening pointed. The judgment on which the estimate of acuity was based was thus reduced to very simple terms, and an objective check was had on its correctness. In the final series of determinations this opening was turned in haphazard order right, left, up, down, and to the four 45° positions; and a correct judgment was required in five out of the eight positions. The breadth of the opening was measured on a micrometer comparator and the visual angle computed. The coefficient of reflection of the test surface was 85%. The preexposure and surrounding fields were made in each case as nearly as possible of the same brightness as

the test surface. An exposure of 1 sec. was allowed for each judgment. The work was done under artificial illumination, the light of a frosted type-B Mazda lamp. The angle of incidence of the light on the test surface was kept constant throughout the experiments. Constancy of position of the observer's eye was secured by biting a mouthboard in which the impression of his teeth had been previously made and hardened in wax.

From 0.001 to 0.1 foot-candle the minimum visual angle changed from 7.143 to 1.213 minutes of arc, a gain of 489% in acuity; from 0.1 to 1 foot-candle it changed from 1.213 to 0.741 minutes of arc, a gain of 63.7% in acuity; from 1 to 5 foot-candles it changed from 0.741 to 0.516 minute of arc, a gain of 43.6% in acuity; and from 5 to 20 foot-candles, it changed from 0.516 to 0.477 minute of arc, a gain of 8.2% in acuity. A curve showing the average results for the four observers is given in Fig. 1. In the table of data for Fig. 1 it will be noted that an acuity of

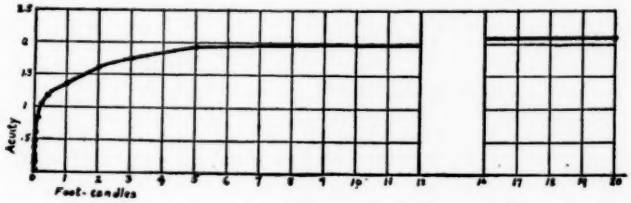

FIG. 1. Showing the effect of increase of intensity of light on acuity (4 observers). Acuity is plotted against foot-candles of light normal to the surface of the test-object.

1, 6/6 or 20/20 was attained at an illumination between 0.1 and 0.2 foot-candle; while at from 5 to 10 foot-candles, the range of intensity perhaps most frequently employed in laboratory testing and in office and clinic work, the acuity fell between 1.94 and 1.97.

In the experiments on eyes slightly defective as to refraction the most practised observer was selected from the former group, and a comparison was made of the effect of change of intensity on this eye normal and made slightly astigmatic in the following ways,— by a +0.12 diopter cylinder; a +0.25 diopter cylinder; a +0.25 diopter cylinder with a correcting cylinder 5° off axis; and a +0.25 diopter cylinder with a correcting cylinder 10° off axis. Artificial astigmatisms were chosen for this work in order that we might know the exact amount and location of the defect, and might have a comparison of the effect on the same eye in the normal and defective condition. They were not chosen with the belief that they are the exact functional equivalent of the natural astigmatism. We are too strongly impressed with the

possibility that the astigmatic eye may progressively acquire some power to compensate for its defect to be of this opinion. The eye was pre-sensitized by 30 min. of adaptation to each illumination. The brightness of the preexposure and surrounding fields was the same as that of the test surface. The coefficient of reflection of the test surface was 85%. The intensities of illumination were 0.015, 0.05, 0.10, 0.25, 0.50, 1, 1.50, 2, 4, 6, 8, 12 and 36 foot-candles normal to the test surface. The results are shown in Fig. 2A and Fig. 2B. In Fig. 2A acuity is plotted against foot-candles and in Fig. 2B percentage-gain in acuity with increase of illumination is plotted against foot-candles. A point of particular significance to be noted in these

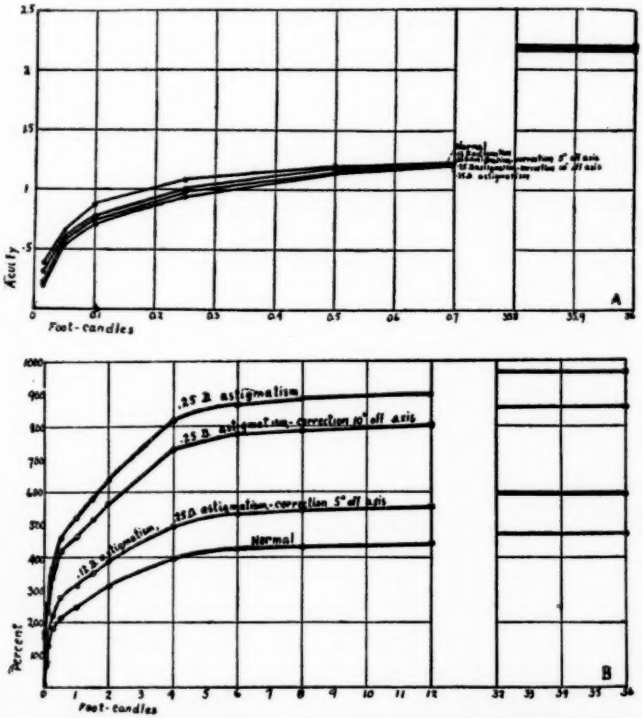

FIG. 2. Showing the effect of increase of intensity of light on acuity for eye with normal refraction and same eye made slightly astigmatic: A, acuity plotted against foot-candles; B, percentage-gain in acuity plotted against foot-candles.

data is that errors of refraction of these magnitudes give a scarcely detectable difference in acuity at the higher illuminations ordinarily used in the laboratory, office and clinic; while at the lower illuminations, around 0.1 to 0.2 foot-candle, they give differences which are readily detectable. Among other things these results indicate why eyes with uncorrected or imperfectly corrected errors of refraction require more light as a comfortable and efficient working minimum than normal eyes; and why corrections and eyes apparently good enough for the higher illuminations are not good enough for work at the lower illuminations. They also raise an important question as to what is the most favorable intensity for the illumination of test charts.

The Intensity of Illumination of Test Charts

The question frequently comes before standardizing committees: At what intensity of illumination should acuity be tested? Our answer would be that it depends for what purpose the test is to be made. There are three obvious applications of acuity testing: vocational selection, diagnosis, and hygiene or welfare of the eye.

In the rating of eyes as to fitness for vocations, the test should be made as nearly as possible at the illumination usually employed in the vocation in question. The study of even a small number of cases shows that the eye can not be given the same relative rating as to acuity at different intensities of illumination. For example, experience has shown in the Navy that only 25 to 30% of the men accepted for the service on the basis of the conventional acuity test at the higher illuminations are able to qualify for the lookout work at night on the bridge of the battleships. Further, in a test of 61 observers made by us, all under 28 years of age and rating 6/4 acuity by the conventional test with 5 foot-candles of light on the test chart, 13% rated below 6/6 at 0.55 foot-candle of light and 33% below 6/6 at 0.2 foot-candle. The acuity of the remainder was 6/6 or better at these illuminations. If speed in the use of the eye at low illumination be added to the requirement, the scatter is very much greater still. The amount of time required just to discriminate 1 min. visual angle in this group of observers, who were all put in the same class by the acuity test at the high illumination, covered a range from slowest to fastest of 1333% at 0.55 foot-candle and of 1443% at 0.2 foot-candle. It is quite obvious that any attempt to rate eyes for vocational purposes at only one or even one order of intensity of illumination is based on a lack of knowledge of the differential effect for different eyes of intensity of illumination on the power of the eye to see clearly.

All will agree, we suppose, that the object in diagnosis is to give the test under the conditions providing the maximum

sensitivity for detecting errors in refraction. A glance at the curves given in Figg. 2A and 2B is sufficient to show that this maximum degree of sensitivity is not obtained at the higher illuminations. For the small uncorrected astigmatic errors represented in these charts the difference in acuity is scarcely detectable at the higher illuminations, but readily detectable at the lower, particularly around 0.1 to 0.2 foot-candle of light. The reason for this is not hard to understand. The details in the slightly blurred astigmatic image can be discriminated at high illumination, but not at low, because of the beneficial effect of increase of intensity of illumination on the resolving power of the retina. This increase in diagnostic sensitivity with decrease of intensity of illumination was further directly tested out as follows. Low artificial astigmatisms were made and corrected. Starting with the proper placement of the correcting cylinder, the axis was shifted from its position by graded changes, ascending and descending series, until the judgment of just noticeable difference in the clearness of the letter B subtending a visual angle of 5 min. was made. This was done at 15, 10, 5, 3, 0.46 and 0.25 foot-candles of light on the test card. The tests were made very carefully. Seven concordant judgments out of ten were accepted as the criterion of just noticeably different in any one set of trials. The results are shown in Table 1.

TABLE I.

Showing number of degrees at which cylinder must be placed off axis to give just noticeable difference of clearness of test-object. Results are based on 7 correct judgments out of 10.

		Degrees cylinder must be placed off axis to give just noticeable difference of clearness	
Observer	Astigmatism	High Illumination (Average of results at 5 and 10 f.c.)	Low Illumination (Average of results at 0.25 and 0.46 f.c.)
H	0.25 cyl. ax.90°	15.0	2.5
B	0.25 cyl. ax.90°	13.5	5.0
L	0.25 cyl. ax.180°	8.5	6.5
S	0.37 cyl. ax. 180°	13.5	4.0
C	0.50 cyl. ax.90°	20.3	9.5
Bs	0.75 cyl. ax.90°	9.0	5.0

In any hygiene or welfare test of the favorableness of working conditions for the eye, the tests should also be made at more than one intensity of illumination. Conditions which are apparently acceptable at the higher illuminations are often far from equally acceptable at the lower illuminations. In all cases it will be found, too, that the sensitivity of the acuity test, whether the purpose be vocational, diagnostic or hygienic, is very greatly enhanced when the procedure is made to include speed, power to sustain, and accuracy, instead of accuracy alone, as is the case in the conventional method of testing acuity.

DATA FOR FIG. 1

Intensity (foot-candles)	Acuity
0.001	0.140
0.005	0.2075
0.010	0.2655
0.015	0.381
0.02	0.483
0.05	0.615
0.10	0.8245
0.20	1.028
0.40	1.158
1.00	1.350
2.00	1.611
3.00	1.743
5.00	1.9385
10.00	1.973
20.00	2.097

DATA FOR FIG. 2 A

Acuity

Intensity (f. c.)	Normal	.12 D. astig.	.22 D. astig.— cor. 5° off axis	.25 D. astig.— cor. 10° off axis	.25 D. astig.
0.015	0.386	0.316	0.316	0.226	0.203
0.050	0.649	0.598	0.596	0.571	0.531
0.10	0.877	0.775	0.775	0.754	0.719
0.25	1.08	1.012	1.004	0.985	0.941
0.50	1.20	1.184	1.184	1.168	1.132
1.00	1.339	1.300	1.294	1.268	1.258
1.50	1.464	1.424	1.424	1.389	1.377
2.00	1.587	1.548	1.554	1.509	1.496
4.00	1.918	1.886	1.895	1.876	1.865
6.00	2.033	1.998	2.012	1.978	1.955
8.00	2.044	2.030	2.030	2.009	1.992
12.00	2.087	2.065	2.065	2.040	2.022
36.00	2.200	2.190	2.190	2.167	2.158

DATA FOR FIG. 2 B

Percent

Intensity (f. c.)	Normal	.12 D. astig.	.25 D. astig.— cor. 5° off axis	.25 D. astig.— cor. 10° off axis	.25 D. astig.
0.015	.....	.....	.....	.....	.....
0.050	68.13	89.24	88.61	152.65	161.58
0.10	127.20	145.25	145.25	233.63	254.18
0.25	179.79	220.25	217.72	335.84	363.55
0.50	210.88	274.68	274.68	416.81	457.64
1.00	246.89	311.39	309.49	461.06	519.70
1.50	279.27	350.63	350.63	514.60	578.33
2.00	311.14	389.87	391.14	567.70	636.95
4.00	396.89	496.84	499.68	730.09	818.72
6.00	426.68	532.28	536.71	775.22	863.05
8.00	429.53	542.41	542.41	788.94	881.28
12.00	440.67	553.48	553.48	802.66	896.06
36.00	469.95	593.04	593.04	858.85	963.05

AUDITORY AND TACTUAL PERCEPTIONS: THE ROLE OF THE IMAGE

By A. Sophie Rogers, Ohio State University

I. Problem and Method

In a previous investigation[1] it became evident that the simple image plays a very important as well as a specific rôle in visual perceptions. In the present series of experiments an elaboration of this problem was carried over to auditory and tactual perceptions. The chief purpose of the experiments was to determine the particular and fundamental relations existing between imagery and perception. These interrelationships fall into three subdivisions: (1) the kinds of images present in auditory and tactual perceptions; (2) the function of the image; and (3) the relation of clearness to the image.

The procedure followed was to start with stimuli as simple and meaningless as possible, and then to proceed with more and more complicated ones. For arousing the auditory perceptions the Wundt sound-hammer was used first for a series of experiments, then a tuning-fork of 256 vs., and lastly a combination of sound-hammer and tuning-fork. The sound-hammer fell five mm. The observer was given 50 stimuli in each series, making a total of 150 reports for each person.

The Wundt sound-hammer was released through dry cells by closing one key of a three-way switch, which released either the sound-hammer alone, or the hammer and fork, or the fork alone, as desired. The striking of the fork and the timing of the duration of its sounding were controlled through the direct current. The tuning-fork was set into vibration by a felt hammer which was drawn down rather forcibly against a magnet when the proper key of the three-way switch was closed. For damping the fork, a "fall away" was constructed, which was drawn down to a magnet simultaneously with the striking hammer. This "fall away" released a timer equipped with a plunger, which lowered into an oil cup. As the plunger lowered, a contact-wire descended into a cup of mercury, thereby completing the circuit through a felt damper, which was drawn down to a magnet and thus checked the vibrations of the fork. For sounding both fork and hammer the middle key of the three-way switch was closed. The intensity of the sound of the fork was controlled by placing two Mazda bulbs, 60 and 40 watt, and a 40 watt carbon bulb in the circuit. All magnets and points of friction were covered with felt to eliminate undesired sounds. *E* sat at the front of the table, where she could operate the apparatus. *O* sat on the opposite side, behind a high upright cardboard, with his eyes closed during the time of each experiment.

[1] A. S. Rogers, An analytic study of visual perceptions, this Journal, 28, 1917, 519.

For the tactual perceptions 25 stimuli[2] of varying degrees of resistance were employed. The same series in the same order was presented three times to each *O* by means of a gravity tachistoscope, adjusted to stimulate *O* for approximately 0.5 sec.

The tachistoscope was provided with an arm-rest and brackets so that the third finger was exposed comfortably for contact. The carriers for the stimuli were of three sizes, 1 x 0.5, 1 x 0.75, and 1 x 1 in. The constant factor of 1 in. represents the length of the stimulus, and considered with the rate of fall of the apparatus determines the duration of the exposure. The series was presented first with the smallest width, secondly with the medium, and thirdly with the greatest. This gives a total of 75 tactual exposures for each *O*.

Throughout the experiments, the following instructions were given to *O*:

"At the 'ready' signal, close your eyes. Two seconds later a sound will be heard (or: a pressure stimulus will be felt). Take the experience passively until a distraction stimulus 'now' is given.

Give a complete account of the experience in terms of:

1. Sensations, images, and affections. Indicate the temporal sequence of the processes.

2. Describe the clearness of the processes as clear, moderately clear, or obscure.

3. Give all meanings in parallel with the processes."

Three *Os*[3] served throughout the experiments. Each reported upon the same number of stimulations, which should be remembered when comparisons are made in the discussion of the results. Before starting upon the main experiments, the *Os* were given preliminary training.

II. Results

A. The kind of image present in auditory and tactual perceptions. This phase of the problem presents three aspects: viz., (1) the similarity and difference in kind of image in auditory and tactual perceptions; (2) the difference in kind and frequency with the increasing complexity of the stimulus; and (3) any variance of imagery shown by different *Os*.

[2]Coarse crinoline; chamois; fine kid; challis; wool whip-cord; brilliantine; heavy satin; coarse ratiné; soft, silk crepe-metior; grosgrain ribbon; fine, tightly woven crinoline; silk netting; organdie; light satin; moray ribbon; chiffon velvet; heavy muslin; coarse sepia paper; soft, rough sepia paper; hard, rough sepia paper; coarse sandpaper; fine sandpaper; velours; bond paper; glazed shellac paper.

[3]The writer is indebted to Louise Ackerman (Senior), Chloie D. McGlinchay (Senior), and J. E. Svoboda (Junior), for their time and patience throughout the experiments.

TABLE I

St.		Indirectly Aroused Processes					Percentages				
		Images				*Sensations*	*Images*				*Sns.*
		Vis.	*Aud.*	*Tact.*	*Total*		*Vis.*	*Aud.*	*Tact.*	*Total*	
H		131	43	18	192	150	38	13	5	56	44
F	No.	127	98	2	227	220	29	22	.4	51.4	49
H & F		156	110		266	271	29	21		50	50
H		10	3	2	1 5	11					
F	%	10	7		17	17					
H & F		12	8		20	20					
	Total	414	251	20	685	641					
	%	32	18	2	52	48					
0.50 in.		68	2	28	98	153	27	.8	11	39	61
0.75 in.	No.	74		32	106	168	27		12	39	61
1.00 in.		74		33	107	170	27		12	39	61
0.50 in.		8	.2	3	11.2	19					
0.75 in.	%	9		4	13	21					
1.00 in.		9		4	13	21					
	Total	216	2	93	311	491					
	%	27	.2	12	39.2	61					

Reference to Table I shows that visual, auditory, and tactual images form the imaginal constituency of both auditory and tactual perceptions. In all this series of experiments but four instances of kinaesthetic imagery were reported. The writer has not considered them for two reasons; first, because their infrequency would not materially modify results or substantiate their existence; and secondly, because there is some doubt about their imaginal character. If we compare the imaginal content of visual,[4] auditory, and tactual perceptions, we find a very decided increase in the kind of imagery corresponding with the sense department stimulated; *i.e.*, when the ear is stimulated, auditory images form 37% of all images, while the same class of images constitute but 2% of the imaginal content in visual perceptions; and when the finger is excited by tactual stimuli, there is a greater percentage (30) of tactual images than in either visual (2) or auditory (3) perceptions.

These facts call forth the old discussion of ideational type. The results of these data point to the conclusion that the kind of image depends more upon the nature of the stimulus and the direction of the attention of the *O* than upon the fact that individuals represent imaginal types. The first part bears out Meumann's point of view[5]: viz., that the type of imagery depends upon the kind of material.

The numerical relations are expressed in the following table. Table II shows the percentage that each kind of imagery constitutes of the total imaginal content within any given type of perception.

[4]The data for the visual perceptions are compiled from the imaginal content recorded in Table I. of the article referred to above.

[5]E. Meumann, *Ueber Oekonomie u. Technik des Lernens*, 1903. *Vorlesungen z. Einführung in die experimentelle Pädagogik*, 1914. Vorlesung 14.

TABLE II.

Sense Dept. Excited	Frequency of Images Expressed in %			
	Visual	Auditory	Tactual	Kinaesthetic
Visual	76	2	2	20
Auditory	60	37	3	
Tactual	69	0.6	30	

These figures also indicate that there is but a slight increase is visual imagery (76%) in visual perceptions over the frequency of the same elements in auditory (60%) and tactual (69%) perceptions. The large percentage of visual imagery in these different kinds of perceptions may be explained by the predominance of attention to visual factors in practically all experiences. Furthermore the visual perceptions display a high frequency of kinaesthetic imagery (20%), while the auditory and tactual present less than 0.1% frequency, but 4 instances in 600 perceptions. This is probably accounted for by the fact that there would be a greater tendency to recall motor concomitants in visual experiences than in auditory and tactual, because of the greater emphasis which is placed upon the former, and the greater frequency of reactions to visual stimuli.

Referring to Table I again, we find, first, that visual images when considered from the aspect of frequency play an almost equally important rôle in auditory and tactual perceptions,—the percentage being practically the same in all series (see column 8 of Table I), with the exception of the series with the hammer, where the percentage is somewhat higher (38). The reason for this may lie in the fact that the sound-hammer represented the most meaningless stimulus employed in the experiments, and that more visual images were called forth in an attempt to satisfy the doubt existing in the minds of the *Os* with respect to the nature of the stimulus. Again, auditory images play about the same part in tactual experiences that tactual images do in the auditory. Secondly, the indirectly aroused sensations are approximately equally frequent in all auditory perceptions and in the three series of tactual experiences, but show a somewhat higher frequency (11 to 17%) in the tactual than in the auditory (see column 12 of Table I).

It is interesting to note that the indirectly aroused sensations in visual perceptions present approximately the same ratio to the imaginal components (47 to 53%) as they do in auditory perceptions (48 to 52%). While the imaginal elements show a greater frequency than the general organic processes in vision and audition, the latter have a much greater percentage (61%) than the images (39%) in tactual perceptions. This seems to indicate that experiences which are more infrequently attended to, when brought to the foreground, involve more motor factors in interpreting and orientating the object.

Table I further indicates the influence of the complexity of the stimulus upon the kind and frequency of the image. As the auditory stimulus varies from the almost "meaningless" noise of the sound-hammer to the familiar sound of a musical tone, the auditory images vary from 43 to 98, or one-third to three-fourths of the frequency of the visual images. When the stimulus is again modified by combining the noise and the tone, the ratio to the visual components is approximately the same as for the tone alone. This seems to point to the tendency of an individual to attend to the more familiar stimulus and to ignore the unfamiliar whenever he meets with the two simultaneously. This fact is confirmed by the introspections of the *O*s. When the fork and hammer were sounded together, the images pertaining to the experience were more frequently related to the tone than to the noise. The complication of the tactual stimuli by increasing the width and thereby increasing the amount of surface stimulated, and thus enhancing the possibility of accuracy of perception, did not present the results found in the case of the auditory perceptions. Tactual stimuli aroused more tactual images than did the auditory stimuli, but the widest stimulus aroused no greater number than the narrowest. In the tactual perceptions, the tactual imaginal factors display about one-third the frequency of the visual elements in each series.

The third part of this phase of the problem to be considered is the variance of imagery with different observers. Table III demonstrates the individual characteristics very plainly.

These distributions show a large percentage of visual images for all *O*s, with a predominance in the cases of M and S. A and M both reported a large frequency of auditory images with auditory stimuli and of tactual images with tactual stimuli. A is a highly trained musician, and one would expect her to experience more auditory images; but it is interesting to see that M, who is not a musician, reported as high a percentage (32%) of auditory images as A (31%). S reported practically no images other than visual. This might be explained by the fact that S attributed a rather definite meaning almost at once to the sound or pressure. There seemed to be lacking that struggling and searching for significance which the other *O*s reported. There seemed to be more "worry" about what it was or meant on the part of A and M, which was correlated with a tendency to ideate the perception.

The following conclusions may be drawn from this part of our study:

(1) In auditory and tactual perceptions, visual, auditory, and tactual images are the dominant imaginal elements. The order in which they are named also gives their order of frequency from greatest to least.

TABLE III.

Obs.	A									M									S								
Images	*Visual*			*Auditory*			*Tactual*			*Visual*			*Auditory*			*Tactual*			*Visual*			*Auditory*			*Tactual*		
Clearness*	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O
H	6	26	2	9	24	1	2	9	7	39	14	3	2	1	2				20	17	4	4					
F	5	33	4	10	35	3			2	38	2		43	4	3				34	6	5						
H & F	2	38	8	3	43	2				60			58	1	3				25	20	3						
Tact. 0.50 in.		24						25		23			2			3			9	11	1						
0.75 in.		24						24		25						8			15	8	2						
1.00 in.		25						25		19	6					4	4		14	9	1						
Total	13	170	14	22	102	6	2	83	9	204	22	3	105	6	8	15	4		117	71	16	4					
Total of Clearness Values		197			130			94			229			119			19			204			4				
Per cent.		47			31			22			63			32			5			97			3				
Grand Total					421									367									208				

*C stands for clear; M, for moderately clear; and O, for obscure. These terms will be used later in the paper.

TABLE IV.

	Frequency										% for every Stimulus									
	Visual			*Auditory*			*Tactual*				*Visual*			*Auditory*			*Tactual*			
St.	I*	E	O	R	I	E	R	I	E	O	I	E	O	R	I	E	R	I	E	O
H	112	30	6	27	15	7		16	3		52	14	3	12	7	3	7	1		
F	53	82		46	25	30		2	1		22	34		19	10	13	.8	.4		
H & F	68	95		74	5	37					24	34		27	2	13				
Totals	233	207	6	147	45	74		18	4											
%	32	28	.8	20	6	10		2	.5											
% of f. in class	52	47	1	55	17	28		87	18											
Tactual																				
0.50 in.	21	50	1		2		4	1	22	1	21	50	1		2		4	1	22	1
0.75 in.	23	53					5	4	25		21	48					5	4	23	
1.00 in.	22	53					5	1	27		20	49					5	1	25	
Totals	66	156	1		2		14	6	74	1										
%	21	49	.3		.6		4	1.9	23	.3										
% of f. in class	29	70	.4		100		15	6	78	1										

*R stands for reproductive; I for interpretative; E for elaborative; and O for orientating.

(2) The kind of image depends to a large extent upon the sense department excited. When the ear is stimulated one should expect more auditory images than when the eye is excited.

(3) The kind of image depends more upon the nature of the stimulus than upon the *O*. There seems to be less ground for adhering to imaginal type than has been supposed. The facts do not indicate any such inherent characteristic.

B. The function of the image. The simple image functions very specifically in the perceptual complex. In the study upon visual perceptions the image served for interpretation, orientation, and appreciation. In this study the functions grouped themselves as reproductive, interpretative, elaborative, and orientating. By the reproductive function is meant the recall of the original sound or pressure without any enhancement of immediate meaning.[8] Thus, M reports:

"I heard the click and tone again with the same intensity and interval as when stimulus was given. The tone seemed to be a little behind the click."

The reproductive image could necessarily be only of the same nature as the sensations aroused directly by the stimulus. Accordingly, in this study, we should find only the auditory and tactual images functioning thus. The interpretative performance of the image is identical with the usage of the term in the previous study. It implies an effort or intent on the part of the *O* to read meaning into the object. The elaborative function is the tendency on the part of the experiencing individual to enhance and enrich the meaning by filling in details and supplementing the directly aroused sensations with images carrying derived meaning. For example:

"The click was that of a little girl striking the pavement after falling from a high window. Visually I saw the place where she landed, her clothing, etc." (S)

"The tone belonged to a phrase of a musical piece which I play. The rest of the melody came in auditory and visual imagery. I could hear the melody and see the score of music. Also I could see and feel my fingers striking the keys." (A)

The orientating function is the same as that referred to in the previous article. The images serve to place the object in time or space with reference either to the *O* or to other objects.

[8]Reproduction, as the term is used in this paper, may be either active or passive on the part of the *O*: *i.e.*, he may purposely reproduce the exact sound or pressure in order further to elaborate the meaning, or images of the original experience may repeat themselves without effort on his part. If the *O* reports that the image is a repetition of the original sound or pressure, whether active or passive, we shall call it reproductive.

With respect to the auditory and tactual perceptions, Table IV. will make clear the results.

In studying this table, it becomes evident that the following conclusions may be formulated:

(1) Since these experiences were auditory and tactual, only the auditory and tactual images can be reproductive. Considering the auditory images themselves, we find that the percentages which recalled merely the direct sensations show a steady increase from those of the "hammer" experiences to the "hammer and fork" experiences; *i.e.*, the more complicated the experience, the greater the tendency to recall the original perception. The reproductive tactual images have approximately the same percentage of frequency for all widths of the stimulus (5%). This indicates that the 1-in. width presented no greater problem to the *O* than the 0.5-in. Also when compared with the reproductive auditory elements, the tactual reproductive images constitute but 15% of all tactual images (see '% of f. in class' under Tactual), whereas the auditory reproductive furnish over half of the auditory images (55%).

(2) With respect to the interpretative function, visual images are of the greatest importance when considered from the point of view of frequency. The highest percentage (52) is in the case of the "hammer" experiences, or the most meaningless and unfamiliar. Between the "hammer" and "fork" experiences there is a difference of 30%. The tone from a fork, being very familiar, would need little interpretation. Between the "fork" and "hammer and fork" perception there is a very small difference, a 2% increase. The 2% increase may be accounted for by the slight amount of questioning which the perception of the click aroused when sounded with the tone. It has been mentioned before that, in these experiences, the *O*s ignored the unfamiliar click and elaborated upon the more familiar and pleasing tone. The percentage of visual images interpreting the tactual sensations is practically the same for the three series (21, 21, and 20%), again indicating an equality in the complexity of the stimuli.

The auditory interpretative images show a greater percentage in the case of the fork (10%) than in that of the hammer (7%). One would expect just the reverse. The result may be accounted for, however, by the fact that one *O* (M) experienced very frequently a primary memory-image of the tone as if it were a tailing-off of the original sound; she reported that there was a tendency on her part to name it. The tone was kept in the focus of attention in order to give it definiteness of meaning. Auditory images serve very little to interpret the "H and F" combination. The auditory images in these experiences were mostly involved in reproducing the sensations, and the inter-

pretation was left to the visual images. Likewise, auditory interpretative images appeared very seldom in the tactual perceptions.

The greatest frequency of the tactual interpretative images (7%) was in the case of the "H" experiences, as was also the case with the analogous visual images. The small percentage of tactual images assisting in reading meaning into the tactual perceptions gives further evidence that the tactual stimuli were not complicated enough to furnish a problem for the *O*.

(3) All classes of images show high frequencies of the elaborative function. This service does not seem to be characteristic of any one particular kind of image. One evident conclusion may be stated, however; namely, that the elaborative function shows a high percentage when the interpretative is low, and a low when the latter is high. That is, whenever the experience is "meaningless" and unfamiliar there is more need for interpretation and not so much chance for elaboration of meaning; and when the object is familiar and easy to apprehend, then the *O* enhances and enriches the immediate meaning with derived significance.

(4) The orientating function presents a very low frequency. In order to place objects in time and place, one must have had more or less experience with analogous objects. The average individual pays comparatively little attention to auditory experiences in comparison with his visual experiences and very, very slight attention to tactual experiences; consequently we find this reflected in the attitude of the *O*s to the various stimuli. The greatest frequency, however, is in the case of the sound-hammer perceptions, just as would be expected. Here the *O* makes the most attempts to interpret and orientate, and the least to elaborate.

(5) Summing up all of these results, the conclusions may be drawn: (*a*) that the more complex the stimulus, the greater is the tendency to reproduce the direct sensations in imagery; and (*b*) that, in passing from unfamiliar to familiar objects, there is first a tendency to interpret, secondly to orientate, and lastly to elaborate upon the immediate meaning.

(6) In comparing these functions with those of the visual perceptions, the images in the auditory and tactual perceptions perform two new functions, those of reproduction and elaboration, while they did not serve to appreciate. The writer read the reports very carefully for anything which might be analogous in the two studies. The fact that the visual perceptions did not show reproductive imaginal elements is probably explicable by the fact that in the auditory and tactual experiences the *O*s found it necessary to reproduce the object in order to interpret it, while in the visual perceptions this seemed to be unnecessary.

Cases in the visual perceptions which verged upon reproduction always had other parts assimilated, and were called forth to question the probability of there being some object which the *O* felt might fit,—which would be interpretative and not reproductive. This may point, again, to a greater ease with visual experiences. Again, with respect to the elaborative function, this is the enhancement and enrichment of meaning without orientation or appreciation. This may be accounted for by past experiences. When one experiences sounds, music in particular, one usually elaborates upon their significance and appreciates them, but is less likely to orientate them in time and place than one does with visual experiences. The sounds presented in these experiments were evidently too simple and unfamiliar to arouse appreciative processes. This latter statement is probably also true of the tactual objects. On the other hand, the visual perceptions did not display any purely elaborative images; *i.e.*, there was no mere tendency to enrich the immediate meaning without interpreting, orientating, or appreciating. This means that, if one has a visual idea of a tree, one does not simply fill in the branches, leaves, and knots, without assigning it to some time or place or without appreciating it. The latter supplements are even the more dominant.

Bartlett[7] has recently conducted an experiment along these same lines, but his approach is different from that of this investigation. He attempts to determine the function of the image through images of memory after various periods of time have elapsed, varying from 15 min. to an indefinite period, not specified in his report. Whereas Bartlett depends upon images of memory entirely, in this study we have considered only those images which are closely assimilated to the perception,—the entire period reported upon being but 2 sec. The methods of procedure also differ widely, Bartlett using very complex stimuli, such as postcards and picture signs (not definitely described, except for the phrase "many of them taken directly from pictographic writing"),[8] with accompanying words to be associated with the signs; while in this experiment we used very simple stimuli, increasing their complexity very slightly as compared with Bartlett's stimuli. All of the stimuli in Bartlett's experiment were presented visually, whereas ours were presented tactually and auditorily; but Bartlett assumes that "visual imagery is typical of direct sensory imagery of all kinds."[9] Our results confirm his assumption. In our interpretations we also differ widely: Bartlett considers the function of the image in its relation to the subsequent reaction, whereas we have considered it in its relations as if looking backward toward the immediate apprehension of the stimulus. As a result of these two points of view, Bartlett finds the image functioning as a facilitation for a type of response, as an aid to a checked reaction, and as an alternative reaction, while we find it serving to reproduce, interpret, orientate, and elaborate.

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[7] F. C. Bartlett, The Functions of Images, *Brit. J. of Psychol.*, 11, 1921,, 320 ff.

[8] *Ibid.*, 321.

[9] *Ibid.*, 324.

TABLE V.

St.	Clearness	1. Distribution: Visual I	Visual E	Visual O	Auditory R	Auditory I	Auditory E	Tactual R	Tactual I	Tactual E	Tactual O	2. % for Each Stimulus: Visual I	Visual E	Visual O	Auditory R	Auditory I	Auditory E	Tactual R	Tactual I	Tactual E	Tactual O	3. No. Clear by Class: Vis.	Aud.	Tact.	4. % Vis.	4. % Aud.	4. % Tact.	5. Total Clear All Classes C	5. Total Clear All Classes M	5. Total Clear All Classes O	6. %	7. Total for Sense Dept. C	7. Total for Sense Dept. M	7. Total for Sense Dept. O	8. % C	8. % M	8. % O
H	C	52	13	2	6	7	2		1	1		27	7	1	3	4	1		.5	.5		67	15	2	35	8	1	84			44						
	M	40	13	4	19	2	4		8	1		21	7	2	10	1	2		4	.5		57	25	9	30	13	5		91		47						
	O	9			1				6	1		5			.5				3	.5		9	1	7	5	.5	4			17	9						
F	C	33	44		29	15	9					15	19		13	7	4					77	53		34	23		130			57						
	M	15	26		14	8	17					7	11		6	4	8					41	39		18	17			80		35						
	O	3	6		3		3		1	1		1	3		1		1		4	.4		9	6	2	4	3	.9			17	8						
H & F	C	51	37		56	3	2					19	14		21	1	.7					88	61		33	23		149			56						
	M	10	48		9	2	33					4	18		3	.7	12					58	44		22	16			102		38						
	O	5	6		3		2					2	2		1		.7					11	5		4	2				16	6	363	273	50	53	40	7
Tactual	C	20	13			2			2	1		20	13			2			2	1		33	2	3	33	2	3	38			38						
0.50 in.	M	1	33	1					2		22 1	1	33	1					2		22 1	35		25	35		25		60		60						
	O		1										1									1			1					1	1						
0.75 in.	C	23	18						5	3		21	17						5	3		41		8	38		7	49			46						
	M		32								24		30								22	32		24	30		22		56		52						
	O		2										2									2			2					2	2						
1.00 in.	C	17	16						2		2	16	15						2		2	33		4	31		4	37			35						
	M	4	36						3	1	25	4	34						3	1	23	40		29	37		27		69		64						
	O		1										1									1			1					1	1	124	185	4	40	59	1

C. The relation of clearness to the image. This phase of the problem again divides itself into three problems: (1) the relation of clearness to the kind of image; (2) the relation of clearness to the function of the image; and (3) the relation of clearness to elaborateness of meaning. This latter problem was left a question in the last article.

A consideration of the first problem may best be considered by interpreting Table V.

By reading caption 3 of the table, it will be seen that the visual images are more frequently clear in all series of the auditory perceptions than moderately clear or obscure. The frequency of clear visual images shows a progression from the "H" through the "F" to the "H and F" experiences of 67, 77, and 88, respectively. The "H" and "H and F" series have practically the same frequency of moderately clear, 57 and 58, respectively, while the "F" series has but 41 cases of moderately clear visual images. The click of the sound-hammer must have been rather confusing and thus have tended to increase the number of unclear processes, while the tone gave no such complications. The very vagueness of meaning seems to have affected the clearness of the process. All of the series present about the same frequency of obscure visual images. Looking across to caption 4, at the corresponding percentages (figured on the basis of each series), we discover that the clear visual images present almost identical percentages (35, 34, 33), as do also the obscure visual processes. The widest discrepancy is among the moderately clear visual images, varying from 30% for the "H" to 18% for the "F" and to 22% for the "H and F" series. Thus the most unfamiliar stimuli arouse the greatest percentage of moderately clear processes. An unknown stimulus, then, has the tendency to increase the number of moderately clear rather than clear or obscure visual processes.

In the case of the auditory images, there is a greater percentage of clear processes when the stimulus possesses more familiar characteristics (23% each for "F" and "H and F") than when the stimulus is unfamiliar (8% for "H"). There is also a slight increase in the percentage of moderately clear elements for the two series having tonal qualities over that for the hammer series. The percentage for obscure auditory images is approximately the same for all three series. Whenever tactual images are present in auditory perceptions they are usually moderately clear or obscure.

Passing now to a consideration of the tactual perceptions, we find that the visual images are fairly evenly distributed in all series between the clear and moderately clear groups. Very few were obscure. This condition may be due to the simplicity of the experience. There were fewer instances of effort on the part

of the *O* to read meaning into the object than in the auditory perceptions; and whenever definite, satisfactory meaning attaches to the situation, the images tend to stand out more clearly. Whatever meaning accrued to the perception came usually without searching for it. Comparing the tactual with the auditory perceptions, this even distribution of clear and moderately clear images would indicate a lack of confusion on the part of the *O*. This is also borne out by the fact that most of the visual images served the elaborative function.

The tactual images in the tactual perceptions, on the other hand, are found almost exclusively in the moderately clear group, showing about an equal distribution for the three series.

Glancing at captions 5 and 6, the frequencies and percentages show that, considering all classes of images in the auditory perceptions, the highest frequency is in the clear groups for the "F" and "H and F" series, while the moderately clear zone is the richest in the "H" series. In the tactual perceptions, the moderately clear processes show the highest percentage in each case. In conclusion, then, the auditory perceptions involved more clear than unclear images, while the tactual perceptions included more unclear than clear. This is explainable by the greater interest the *O*s displayed for the auditory presentations, especially those involving the tone.

The second phase of this part of the problem, *i.e.*, the relation of clearness to the function of the image, may be explained by reference to captions 1 and 2 of Table V. for comparisons between the various series and to Table VI. for the composite compilations.

The visual images which serve the interpretative function have higher percentages of clear than of moderately clear and obscure together, both in auditory and tactual perceptions. Thus, wherever effort is involved, the processes are pressed into the focus of attention. Only in the "H" series does the percentage of the moderately clear approach that of the clear. The auditory images performing the same function show the same relationship that the visual images do. The tactual interpretative images are mostly unclear in the auditory perceptions, but usually clear in the tactual perceptions.

In the case of the elaborative function, the visual images show no consistent distribution among clear and unclear groupings in the auditory perceptions. In the "H" series the distribution is equal (7% of each); in the "F" series, the clear outnumber the unclear (19% clear, 11% moderately clear, and 3% obscure); while in the "H and F" series the reverse is true,—the unclear surpass the clear (14% clear, 18% moderately clear, and 2% obscure). The higher percentage of clear than unclear in the "F" series is due to the high frequency of clear visual

elaborative images of one *O* (M) in this series. She was particularly pleasantly affected by the tone, and elaborated it into church bells, chimes, etc. In the tactual perceptions, the distribution of the percentages is consistent throughout the three series, the highest percentage being in the moderately clear, the second highest in the clear, and the lowest in the obscure region. Each series possesses approximately the same proportion. The distribution of the first is typical: 13% clear, 33% moderately clear, and 1% obscure. The auditory and tactual images of the elaborative function in the six series show practically the same distribution as the visual do in the tactual perceptions.

The auditory images serving the reproductive function display the greatest frequency of moderately clear for the "H" series, the next greatest frequency of clear processes, and the least of the obscure. The relationship of the clear and moderately clear in the "F" and "H and F" series is the reverse of that in the "H" series. The tactual images functionating to recall the original sensations are about evenly distributed among the clear and moderately clear fields of attention.

Six out of the eight instances of the orientating function of both visual and tactual images are moderately clear.

In Table VI. a composite of each function of each kind of image has been arranged according to clearness values, so that this table shows at a glance the relationships between clearness and the functions of the various kinds of images.

Summing up all that has been said about this phase of the problem, and setting forth the implications of Table VI., the following results may be stated.

(1) The image involved in the reproductive function is more frequently clear than unclear. No additional images or indirectly aroused sensations are necessary for mere recall, so that the clearness of these images is not reduced by complication of the experience.

(2) The interpretative images are more frequently clear than unclear, with one exception, when tactual images serve to interpret auditory perceptions.

(3) The images which serve to elaborate the meaning of the perception lie without exception for the greater part in the background of attention. In only one instance are there more clear than moderately clear cases, *i.e.*, in the "H" series; but when the obscure are added to the moderately clear, the unclear outnumber the clear.

(4) The orientating function displays the greatest frequency in the moderately clear region.

TABLE VI.

Stimulus	Function	Distribution												Percentages											
		R			I			E			O			R			I			E			O		
	Clearness	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O
Auditory	Kind of Images: Visual				136	65	17	94	87	12	2	4					20	9	2	14	13	2	.3	.6	
	Auditory	91	42	7	25	12		13	54	5				13	6	1	4	2		2	8	.7			
	Tactual				1	8	7	1	1	2							.1	1	1	.1	.1	.3			
Tactual	Visual				60	5		47	101	4		1					19	2		15	32	1		.3	
	Auditory				2												.6								
	Tactual	7	5		4	1		2	71			1		2	2		1	.3		.6	23			.3	
Total for Auditory		91	42	7	162	85	24	108	142	19	2	4													
	%	13	6	1	24	12	3	16	21	3	.3	.6													
Total for Tactual		7	5		66	6		49	172	4		2													
	%	2	2		21	2		16	55	1		.6													
Total for Aud. & Tact		98	47	7	228	91	24	157	314	23	2	6													
	%	10	5	.7	23	9	2	16	31	2	.2	.6													

The third subdivision of this phase of the problem undertakes to determine the relation of clearness to the elaborateness of meaning. After the discussion of the relation of clearness to the function of the image, it is very evident that, when the meaning is slight, unfamiliar, confusing, and difficult to grasp, the images involved in such complexes serve more for reproduction and interpretation, and by virtue of the very effort put forth become clearer. When the meaning, on the other hand, is more highly derived, familiar, explicit, and easy to comprehend, the images are more likely to serve for elaboration, arise without so much effort on the part of the *O*, possess a higher percentage of frequency, and are mostly moderately clear.

Thus the conclusion may be drawn that images are clear, but less numerous, when significance is slight; while, on the other hand, they are numerous, but moderately clear, when meaning is more elaborate.

III. Summary and Conclusions

Summarizing, we may say that auditory and tactual perceptions involve mostly visual, auditory, and tactual images, and that the percentage of frequency of the kind of image depends upon the nature of the stimulus. Images furnish approximately one-half of the indirectly aroused processes (52%) and organic sensations the other half (48%) in auditory perceptions, while in the tactual perceptions the organic processes form 61% and the imaginal 39%. An increase in the complexity of the stimulus is correlated with an increase in the kind of image corresponding to the directly aroused sensations.

These simple images render very definite services to the auditory and tactual perceptions; namely, those of reproduction, interpretation, orientation, and elaboration. Since these were auditory and tactual perceptions, the reproductive function was limited entirely to auditory and tactual images. The interpretative function is more characteristic of visual than of auditory and tactual imaginal elements. So also are the elaborative and orientating functions. The variance in frequency of these functions depends upon different factors; the function of reproduction becomes more frequent with the complexity of the stimulus, while the other three functions depend upon the degree of familiarity of the stimulus,—the less familiar involving first the interpretative, secondly the orientating, and the more familiar the elaborative function.

With respect to clearness, we find that most of the visual and auditory images tend to be clear, while the tactual tend to be moderately clear. The kind of image in relation to clearness can scarcely be considered without the function. Whenever the image serves the interpretative and reproductive functions, it

is more frequently clear than unclear; while, when it functionates elaboratively or orientatively, it is more frequently unclear. Another very definite relationship between the clearness of the image and the perception is that, when the meaning of the perception is hazy and vague as in the case of the "hammer" experiences, or slight as in the tactual experiences, the images on the whole are unclear; while, in instances where the meaning is more elaborate and derived, more images are clear as in the "fork" and "hammer and fork" experiences (see captions 5 and 6 of Table V.). This general relationship is modified, however, when the function of images is taken into account, as has been indicated above; interpretative images, which are more closely attached to perceptions with vague meaning, are more frequently clear; and orientating and elaborative images, which are attached to perceptions of derived significance, are more frequently in the background of attention. These two statements are reconciled by the fact that reproductive images are more frequently clear than moderately clear, and that their greatest frequency is in perceptions of elaborate meanings and not in perceptions with slight significance. This accounts for the relation between clearness and meaning when we consider the total number of images.

In final conclusion, it may be stated that the kind, complexity and familiarity of the object arousing a perception could be fairly well predicted from the kinds of images involved, the frequency of the clear and unclear processes, and the function of the image.

OBSERVATIONS ON TAKING PEYOTE (*ANHALONIUM LEWINII*)

By Samuel W. Fernberger, University of Pennsylvania

The following observations were made on taking Peyote (*Anhalonium Lewinii*). The drug is one of the cacti frequently and popularly called Mescal Buttons. The chemistry of the drug is imperfectlv understood, but the active principle seems to be one of the alkaloids.[1] The plant is found only in parts of the Rio Grande valley on the Mexican side. It is frequently used by the local Indians as part of their religious ceremonies. Within the last twenty-five years a rather widespread Peyote cult has grown up among the Indian tribes just west of the Mississippi basin, and this cult has reached as far north as Missouri at least. As a matter of fact, the use of the drug among the Indians has become so widespread that several of the states have taken active measures to try to prevent its use.[2]

The reason the Indians have made its use part of their religious ceremonies is that the eating of the drug is followed by the appearance of colored images. A number of cases are described in the literature, of which we mention two only as typical. Mitchell[3] gives a very interesting account of his own experiences and of those of a friend. Mitchell's own experience appears to have been as vivid as any that is recorded in the literature,—whether or not owing to the well-known ability in verbal description of the late Dr. Mitchell, we cannot say. The friend had colored visions also, but apparently not so brilliant as those of Mitchell. The other report is that of Prentiss and Morgan, mentioned above. In all they report six experiments on five subjects. One subject submitted to taking the drug twice, in spite of what Mitchell declares: "These shows are expensive. For two days I had headache and for one day a smart attack of gastric distress. This came after the first dose and was most uncomfortable. The experience, however, was worth one such headache and indigestion, but it was not worth a second."

The summary of these seven cases seems to show most clearly that visual manifestations occur after ingesting the drug. Great individual differences are to be noted in the vividness, intensity, saturation, complexity and duration of the visual phenomena. In every case they are colored, and in every case the colors seems to be in movement and constant change. The manifestations may take the form of known objects or may be of abstract geometrical shapes. All subjects agree that the visions are obtained only with the eyes closed, and that they disappear when the eyes are opened even

[1]For a description of the chemical reactions and physiological effects on animals, *cf.* L. Lewin. *Anhalonium Lewinii, Therapeutic Gazette*, 4, 1888, 231-237.

[2]For a description of the Peyote cult ceremony, *cf.* D. W. Prentiss and F. P. Morgan. *Anhalonium Lewinii* (Mescal Buttons): A Study of the Drug with Especial Reference to its Physiological Action upon Man, with Report of Experiments, *Therapeutic Gazette*, 9, 1895, 577-585. Also M. R. Gilmore. Uses of Plants by Indians of the Missouri River Region, 33*d Annual Report of the Bureau of American Ethnology*, 1919, 104-106.

[3]S. Weir Mitchell. The Effects of *Anhalonium Lewinii* (The Mescal Button), *British Medical Journal*, 2, 1896, 1625-1629.

in a darkened room. Several of the subjects note that visual after-images seem to be of longer than the usual duration. Most of the subjects also report a distortion of time, which seems very much prolonged; and in one case there was a distortion of visual space. Both Mitchell and Prentiss and Morgan believe that these visual manifestations are of cortical origin and are to be identified as visual images.

Thus far no reports by psychologists trained in observing mental processes have appeared in the literature. The one attempt is that of James,[4] who remarks that he ate one button (obviously too small a dose), became ill, had no visions, and discontinued the experiment. It seemed worth while to try the experiment, if only to determine whether the visions are to be identified with visual memory-images. It has been demonstrated by continued introspection, both in and out of experimental settings, that I am almost entirely lacking in visual imagery. It therefore seemed probable that, if these manifestations are imaginal in character, they would not be present in my case.

The experiment was conducted at Clark University on February 27, 1920. I wish to express my thanks to Professor Edwin G. Boring for his kind assistance and suggestions during the course of the observations. The reports were taken down in full by a stenographer. During a large part of the experience I was lying on a lounge with my eyes blindfolded with black velvet. A total dose of 39 grams, amounting to six large buttons, was ingested. The dried buttons were chewed and swallowed. This was a lengthy affair, inasmuch as they were intensively bitter and very nauseous. The first dose of 13 grams was taken at 12 noon; 13 grams more at 12.30; and the final 13 grams at 1.30 p. m. During most of the observation I was bothered with stomachic aches and pressure in the abdomen, and later with continuous frontal and occipital headache.

By 1.06 p. m. I observed that, on closing the eyes, both positive and negative visual after-images seemed to be relatively more intensive and of longer duration than usual. About this time I reclined and bandaged the eyes. At 1.43 a spot of white light of low intensity appeared about the center of the horopter. From then until 2.58 there were intermittent color effects. Colors from violet to orange appeared, but seldom red. The colors were rarely of a high degree of saturation. Two spatial forms were present,—one in the form of an arc in the upper half of the horopter, and the other in the form of lines down the center of the horopter. The colors seemed to be in constant movement. They did not seem to be affected when I opened the eyes inside the blinders, which were entirely light-proof.

At 2.58 I removed the blinders. After light-adaptation had progressed, I discovered an inability to hold fixation on any object for more than an instant. At 3.07 I made a chance discovery. While smoking a cigarette, I observed that the lighted tip, seen in indirect vision, was relatively much clearer and more intensive and saturated in color than normal. This fact was verified; and it was very noticeable that colors in the periphery of the visual field had a more than normal degree of saturation and clearness. This did not appear to be true, at this time, of colors seen in foveal vision.

At 3.22 the blinders were replaced, and the color-effects began immediately. During this period colored circles appeared, always spinning in a counter-clockwise direction.

At 3.48, over two hours after the final dose, a new series of phenomena become apparent. These phenomena were of such an unexpected sort and were so interesting that perhaps they have vitiated, to an extent, the observations. At any rate the blinders were not resumed and no further mention is made of retinal phenomena. These new phenomena had primarily

[4]W. James. Familiar Letters of William James, *The Atlantic Monthly*, 1920, August, 171.

to do with kinaesthetic sensations. At 3.48 I noted that dictation seemed to be very much slower than normal, although the stenographer assured me that my speech was as rapid as usual. At 3.54, when moving my shoulder, I was aware of clearer kinaesthetic sensations than I had ever known before in my experience. The muscular, and especially the articular, sensations were tremendously clear, but not more intensive than normal. This, in spite of the fact that I am of a kinaesthetic type and therefore used to attending to sensations of this modality. The blinders were then removed, and the parts of my body, seen visually, seemed to be very far away. I extended my arm and fixated the finger-tips, and then stretched the arm away from me. The hand seemed to move to a distance of at least ten feet. The stenographer moved her head and shoulders back a few inches, and she seemed to be drawing back many times that distance. This distortion of space was present for both monocular and binocular vision.

Experiences of this sort lasted several hours. During this period I was in what must have been very close to a state of dual personality. I was able to make perfectly rational and normal verbal judgments that were totally at variance with the immediately present sensory cues. I could say that a certain calendar was only actually four inches long although it appeared to be many feet. So far as one could analyze, the only conscious processes which would account for this state were a greater clearness of all sensations, but especially of those of a kinaesthetic sort. This would account for the distortion of visual space at least. If our judgment of depth is dependent on sensations from changes in parallax, then a great increase in the clearness of these sensations would give the illusion of magnifying visual space. Such an analysis would account for the magnification of kinaesthetic space as well.

My state, at this period, can best be described as a supernormally clear focus of attention (which, however, was very rapidly changing) with practically no background of consciousness. The increase in clearness was quite evident in sensory modalities other than the kinaesthetic. I was aware of throat-noises when a person a number of feet away from me swallowed; of color shadows in a magazine cover, and the like; in a word, of stimuli which are normally well above the sensory threshold but which usually pass unnoticed.

Accompanying this distortion of space was also a distortion of time. A given period of time seemed very much more extended than normally. Speech seemed slow. Walking became a ponderous affair. In walking I felt successively sensations from the muscles and joints involved. I became aware at each step, for example, of sensations due to the curling of the toes. If the clearness of the sensations accounts for the distortion of space, the very rapidly changing focus of attention would readily account for the distortion of time. We naturally and normally judge a period of time by the 'filling', by the number of processes that have claimed the center of attention. Under the influence of peyote this filling, for a given unit of time, is much increased, and so the period seems extended.

These reactions were of an unexpected sort and could not have been anticipated from a reading of the literature. They may have been due either to the fact that I am non-visual or to the fact that I took a larger dose of the drug than is elsewhere recorded in the literature. As a result we were quite unprepared to make sensory tests. Several tests of a rough sort were tried. Apparently my ability to judge brightnesses of greys was not enhanced. My after-images of the James waterfall and of the Mach spiral were normal. Stereograms were normal. An absolutely restful insomnia lasted throughout the night. I worked the next day without, apparently, any disastrously ill effects. It was at least forty-eight hours before the dilatation of the pupils of the eyes and other physiological effects had entirely disappeared.

The state I have just described seems more like that reported for hasheesh or *cannabis indica* than for peyote. The exception is that the illusions following the use of these two drugs are described as being accompanied by a 'feeling of superiority'. In the present experience I had a definite feeling of inferiority. Mitchell, however, reports the opposite: "Yet the sense of increased ability was so notable that, liking to test it, and with the commonsense belief in its flattery, I took up a certain paper on psychology, which a week before I laid down in despair. I grieve to say that it was less to be comprehended than ever."

The above experiment was made in an effort to determine whether or not the visual manifestations following the use of peyote are imaginal in character. In that respect the experiment was a failure. Visual colored manifestations occured in my case in spite of the fact that I am normally almost totally devoid of visual images. These phenomena were, however, much less intensive and saturated in my case than would appear from other reports in the literature. The colored phenomena appeared only on a dark field, either with the eyes closed or with the eyes opened inside a light-proof bandage. The crucial test, as Mitchell has already pointed out, can be made only on a congenitally blind subject. Up to the present time I have been unable to induce such a subject to undergo the experiment. I shall be glad to supply the drug to anyone who can procure the proper subject.

Owing possibly to the fact that I took an especially large dose of the drug, I experienced a distortion of space and time not recorded for this drug but apparently resembling that obtained with hasheesh or *cannabis indica*. All sensations appeared to be greatly enhanced in clearness but not in intensity or, in the case of colors, in saturation. The state can be described as a very clear but rapidly changing focus of attention. The increased clearness seems to account for the distortion of space; the rapidly fluctuating focus of attention seems to account for the distortion of time.

ERNST MEUMANN 1862–1915

By G. Stoerring, University of Bonn

I am glad to comply with the Editor's request to say something of the life and work of my friend, the late Ernst Meumann.

Meumann, the son of a Lutheran pastor, was born in Uerdingen bei Wesel on the Lower Rhine, August 29, 1862. After finishing his high-school course he matriculated (Easter, 1883) at the University of Tübingen, and devoted himself to the study of theology and philosophy. He later attended the Universities of Berlin, Halle, and Bonn, until Easter, 1887. He passed successfully the two theological examinations, and in 1891 obtained at Tübingen the degree of doctor of philosophy with a thesis on the Law of Association and Reproduction. He then turned to the study of experimental psychology, and entered Wundt's Institute at Leipzig. In 1894 he became *Privatdozent* in philosophy at Leipzig; his dissertation was entitled An Investigation of the Psychology and Aesthetics of Rhythm. In the years 1893 to 1896 he published his famous studies in the psychology of the temporal consciousness. In 1897 he was called to the University of Zürich as professor of philosophy, psychology and education. In 1905 he accepted a call to the University of Königsberg. He then occupied, in quick succession, chairs of philosophy at the Universities of Münster, Halle, and Leipzig, and finally settled down in the *Vorlesungswesen* at Hamburg, where he died of pneumonia, April 25, 1915.

I first met Meumann when we were students at Halle. We had kindred scientific ideals and soon formed a close friendship. At that time he was mainly interested in problems of metaphysics; he had as yet no thought of experimental psychology and education.

Meumann had been very religiously brought up, and religious beliefs and modes of thought has taken strong hold on him. He was now to fight the battle between faith and knowledge. He found himself obliged, though it cost him a serious struggle, to give up one dogma after another. He had meant to become a teacher of religion, and with that end in view to make himself a member of some philosophical faculty. Presently, however, when he realized that he had travelled too far from the ideas of orthodox Christianity to be able, with good conscience, to undertake the task of religious instruction, he reluctantly gave up his original plan. We then decided to take up together the study of medicine; for we had come to the conclusion that psychology is the necessary condition of any wholesome philosophising, and that certain medical studies are the essential precondition of successful work in psychology. Meumann, unfortunately, could not accustom himself to the odors of the dissecting room, and was therefore unable to complete his medical course. He sought accordingly to gain from books the knowledge of anatomy, physiology, and psychiatry that he judged necessary for psychology, and in fact acquired an exceptionally thorough knowledge of physiology.

Meumann had a quite extraordinary enthusiasm for scientific investigation, together with an unusual tenacity of purpose, so that he managed, despite a poor physical constitution, to get through an astonishingly large amount of work. To this intense interest in science he added an enthusiasm for social ideals, which was upheld by his strictly ethical view of human life and a truly Kantian conscientiousness. His ethical views were influenced by his hope in the moral progress of mankind.

Meumann had also a living interest in aesthetics. It was delightful to be with him on country walks and to share his keen appreciation of the beauties of nature. The service of art was for him a necessity of life. His knowledge of the history of art covered a wide range, so that he could appreciate a work of art from the artist's standpoint. He took an eager interest in the artistic furnishing of his own home.

He was strongly averse from everything mean and unworthy. His whole nature was ingenuous and sincere. He gave but little thought to the affairs of everyday life; his concern was rather with the welfare of mankind at large; and his private circumstances often suffered in consequence. It might have been better if he had paid a little more attention to money matters.

All the traits of his character and personality were drawn in large and noble lines. His manners were simple; he showed no trace of self-sufficiency; he never posed as a famous man. He bore himself modestly, despite his great achievements, and was nearly always dissatisfied with what he had done.

His scientific work lay in the fields of psychology, education and aesthetics.

Meumann's psychological investigations reveal him as a psychological experimentalist of the first rank and as a master of psychological discussion. These qualities appear even in his early articles on the temporal consciousness, which, as Wundt rightly remarks, stand among the very best that the literature of experimental psychology has to show.

We owe to Meumann and his pupils a series of admirable researches on the function of memory. Meumann drew a sharp line of distinction between immediate and permanent retention, and formulated the relations of dependence that characterise immediate retention. He also, in collaboration with his pupils, made additions of our knowledge of permanent retention and of ideational types. Further, and as a still more important contribution to this chapter of psychology, he and his pupil Ebert carried out pioneer investigations of the relation of general to special practice; discovered the fact that any special practice means the concomitant practice of related activities; and offered a competent defence of the hypothesis of a general practice, over and above the concomitant practice manifested by the part-activities, identical or similar, of a special practice.

Next, we owe to Meumann an increased knowledge of types of attention, which he first came upon in his study of immediate retention and which he later made the subject of tachistoscopic experiments. I mention here only one point, his discovery of the difference between total and discrete attention. With the one attitude of attention—so he found in his work on immediate retention—the stimuli exposed constitute in the observer's consciousness a whole, which before recitation he possesses in its totality and which, when recitation is called for, he gives forth with all speed. With the other attitude of attention the apprehension of the series takes a different course; it is now the rule that only the member of the series actually exposed finds representation in consciousness, and that before recitation the first and last syllables, but no more, are in the observer's mind; recitation proceeds relatively slowly and in characteristic fashion.

I will end this brief account of Meumann's special researches by saying a word of his work on feeling and its concomitant phenemena. The pneumographic studies, which he made in collaboration with his pupils in order to a characterization of state of feeling by breathing-curve, are excellent of their kind. But he went further; he directed investigations that are of cardinal importance for the theory of feeling; and in his own articles on the sensitivity of the internal organs he turned to account the experiences of the surgeon and the pathologist for a theory of organic sensation.—

In his book on Intelligence and Will Meumann attacks problems of greater psychological complexity. Here he discusses intelligence and will each for itself, in its various modes and degrees, and each also in its relation to the other; nor does he fail to raise the question of the significance of intelligence and will for personality and for human life. The book abounds in keen psychological observations of the individual character, and in sober interpretation of these complex observations. We have been told, in recent years, that modern psychology is simply a branch of natural science, and that it has to do only with the simplest things of mind; and we have been told that this modern psychology must be supplemented by another, philosophically orientated psychology, which shall make no claim to carry us back to the ultimate elements of the mental life. To those of us who are repelled by such a doctrine, Meumann's contribution to the psychology of personality, which draws its materials from the whole complex round of life, is a work of outstanding value.

There can, then, be no doubt that Meumann takes his place among the leaders of contemporary psychology. In the field of experimental education, to which we now turn, he was, without question, the principal figure. It is true that studies in experimental education had been published before his day. But he did so much to further and extend this type of scientific enquiry, so much to organize and stabilize its results, that we must hail him the founder of experimental education as a special discipline. At the same time, he did not regard experimental education as the whole of education; he sought rather to relate it to the achievements of the great educators of the past. Moreover, he believed that education is concerned not only with the systematic guidance of the child in school but also with public instruction beyond the limits of the schoolroom. Its task, in this wider sphere, is to serve the cause of civilization at home and to advance international thought upon political and social problems. Public instruction is thus to be built up on a national foundation, but its national character must not render it blind to international interests. In December, 1914, during the Great War, Meumann wrote in the preface to the third volume of his Lectures on Experimental Education: "I should like to repeat here what I have said in the concluding sentences of the book,—that my occupation with questions of education has brought me again and again to realize how strong and how far-reaching are the intellectual and moral ties between the leading civilized nations of our time. In the field of education all these nations pursue the same ideal ends, and community of educational problems far outweighs national and local differences and divergences of opinion; so that there is no other sphere of human endeavor that affords such manifold opportunity for friendly exchange and mutual give-and-take. Perhaps, then, this book of mine, which has already found many friends abroad, especially (and, I think, significantly) in Austria-Hungary and the United States of America, may in some small measure help toward strengthening the intellectual ties that bind the civilized peoples together and toward the reconciliation of the nations now at war; for all alike have the same work to do in the moral education of youth."

Meumann assigned to aesthetics a fourfold task. It has to carry out a psychological investigation of artistic production and of aesthetic evaluation; it has to establish aesthetic norms; and it has to treat of art as a factor in the history of civilization. In his System of Aesthetics Meumann made a psychological study of aesthetic evaluation in closest connection with the facts of experimental aesthetics, and proposed a carefully graded scale of aesthetic judgment. At the highest level of aesthetic judgment of a work of art we have sympathetic feeling for, and sympathetic understanding of, the aim of the artist's labors. Meumann's psychological discussion of artistic production is highly original and important. Artistic production

depends, first, on some experience that stirs the artist on the emotive side; secondly, on the urge to give expression to this experience; and thirdly on the endeavor to cast this expression into the form of a work of art. The motive to express and the motive to represent are discussed with rare insight. It is shown that the motive of representation sets limits to the motive of expression, limits which vary with the various modes of artistic production (laws of style in art). Painting, for example, offers many difficulties of representation that are foreign to poetry, and conversely. It is shown further, with all the force of demonstration, that the varying relation of the motive of expression to the motive of representation or of form determines the appearance of different schools of art, and that in modern art the motive to express takes precedence of the motive to form.

In his treatment of the norms of aesthetic evaluation Meumann laid down rules which are. as he said, of a quite general character and therefore —since they derive from the nature of art itself and from considerations directly suggested by that nature—escape the arbitrariness of the particular art-school or artistic authority. His use of psychological facts in the establishment of these aesthetic norms brought on him, in philosophical quarters, the reproach of psychologism. I cannot grant its justice. In the sphere of logic, where the argument deals with truths of reason and not with simple truths of fact, recourse to psychology, which is concerned only with truths of fact, is plainly impermissible. When, however, it is a question of the validity of aesthetic or (within limits) of ethical evaluations, then the introduction of psychological facts is not only harmless but actually necessary.[1]

Finally, I must not omit a reference to Meumann's executive capacity. He founded and edited the *Archiv für die gesamte Psychologie*, as well as his special educational *Zeitschrift*.

The death of Meumann removes a man who had rendered enduring service to psychology, education and aesthetics and to the practical reform of the teaching profession. With his brilliant endowment and his tremendous power of work he would, had life been spared, have added worthily even to his own great achievement.

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[1] I have discussed this point in *Sittliche Forderungen und die Frage ihrer Gültigkeit*, 106 ff.

WILLIAM HALSE RIVERS RIVERS 1864-1922

By F. C. Bartlett, Cambridge University

On June 3 last year I was walking through the grounds of St. John's College, here in Cambridge, when I met Dr. Rivers returning from a stroll. He was full of energy and enthusiasm, and began at once to talk about certain new courses of lectures which he proposed to deliver at the Psychological Laboratory during the present year. On the evening of the next day I heard that he was dangerously ill. As I approached the College on the morning of June 5 I saw the flag at halfmast. He had, in fact, died in the early afternoon of the preceding day. Never have I known so deep a gloom settle upon the College as fell upon it at that time. There was hardly a man—young or old—who did not seem to be intimately and personally affected. Rivers knew nearly everybody. As Praelector of Natural Sciences at St. John's he interviewed all the science freshmen when they came first into residence and, in an amazing number of cases, he kept in close touch with them throughout their Cambridge career. Everybody who came into contact with him was stimulated and helped to a degree which those who are acquainted only with his published works can never fully realize.

Rivers was born in 1864, and in due course went to St. Bartholomew's Hospital where, at the age of twenty-two, he qualified to practise medicine. Already his attention had turned in the direction of psychology. As House Physician at St. Bartholomew's he read several papers before the Abernethian Society on hysteria and neurasthenia. In 1891 he was appointed House Physician at the National Hospital for the Paralysed and Epileptic. Here it was that he met Dr. Henry Head, with whom he was later to collaborate in the important work on cutaneous sensibility. He had become interested in vision, and largely under the influence of Head, who had worked with Hering at Prague, he adopted with enthusiasm Hering's theories of color vision.

In 1893 Rivers was invited to Cambridge by Sir Michael Foster, to lecture on the physiology of the special senses, and in 1897 a movement, initiated many years before by Professor James Ward and Dr. Venn for the official recognition by the University of Experimental Psychology, produced its definite result, and Rivers was made University Lecturer of Physiological and Experimental Psychology. He was thus the first officially recognized teacher of experimental psychology at Cambridge. and the first person in England to plan and carry out a systematic course of practical teaching in the subject.

The year following he went with the Cambridge Anthropological Expedition to the Torres Straits. This was the first step of the second period of his career—the ethnological period, as it may be called. For some years yet, however, the greater part of his work was concerned with the psychology of the special senses, and particularly with that of vision. A glance at the bibliography of Rivers' published writings shows that, down to 1908, experimental work in psychology still occupied a large amount of his time. During this period he was carrying out his experiments on the influence of alcohol and other drugs on fatigue—published in 1908 as the Croonian Lectures to the Royal College of Physicians—and his "Human Experiment in Nerve Division", with Dr. Head. Meanwhile an expedition to Southern India in 1902 had produced a number of papers on the Todas

and, finally, his book upon the same community. In 1904, with Professor James Ward and some others, Rivers founded the British Journal of Psychology of which he was at first joint editor.

From 1908 till the outbreak of the late war Dr. Rivers was mainly preoccupied with ethnological and sociological problems. Already he had relinquished his official post as Lecturer in Experimental Psychology in favour of Dr. C. S. Myers, and now held only a lectureship on the physiology of the special senses. By degrees he became more absorbed in anthropological research until, several years before his death, he resigned his lectureship, and thenceforward held no University appointment. But though he was now ethnologist rather than psychologist he always maintained that what was of value in his work was due directly to his training in the psychological laboratory. In the laboratory he had learnt the importance of exact method; in the field he now gained vigor and vitality by his constant contact with the actual daily behavior of human beings.

In 1908 Rivers made his first journey to Melanesia. The material and interests which the voyage gave him occupied practically the whole of his attention until 1914, when his great work entitled "A History of Melanesian Society" was published. In that year he made a second journey to Melanesia, returning to England in March 1915, to find that war had broken out.

Now came the third period of his life, in which all his old enthusiasm for psychology returned, intensified and strengthened by a determination to keep his knowledge in the closest possible touch with real life. He now carried out a very large amount of neurological work, first with men and officers who were suffering from various psycho-neuroses of war, and in the second place as Psychologist to the Royal Air Force. In this work he was extraordinarily successful, for throughout the whole of his life he possessed a remarkable power of attracting sympathy and confidence.

In the last five years of his life Rivers was amazingly active. He published a large number of papers, some on ethnology, some on neurology, but most on psychology. All of them were marked by great originality of ideas and a new and brilliant incisiveness of expression. It was now that he wrote "Instinct and the Unconscious" (published in 1920), and prepared for publication no less than three other books, two of which—"Conflict and Dream", and "Psychology and Politics"—have now been published, and the third of which—"Principles of Social Organisation"—is in the press. All of these were given first as lectures in Cambridge, and none of the large number of students who heard them is likely to forget the frankness with which Rivers was prepared to discuss difficulties, or the readiness with which he admitted the possibility and importance of other points of view than his own.

But his literary activities were only a small part of his crowded life during these days. He was a great influence in all the administrative life of his College; entertained constantly; regularly stimulated and directed informal discussions on any kind of subject among undergraduates; and took the keenest interest in the development of the fine Psychological Laboratory which had been founded at Cambridge by the efforts of Dr. C. S. Myers. He became more widely known than ever before, lecturing frequently upon psychological and sociological topics in many places. He often spoke to me with great appreciation of the stimulus he obtained from his visit to America in 1921.

Honours of many kinds had been showered upon him. In 1902 he was made a Fellow of St. John's College, and Praelector of Natural Sciences in 1919. He held the post of Croonian Lecturer to the Royal College of Physicians in 1906, and the Fitzpatric Lectureship in 1915-16. The Fellowship of the Royal Society was given him in 1908, and in 1915 he was awarded a Royal Medal. He was President of the Anthropological Section of the

British Association in 1911, and of the Psychological Sub-Section in 1919. In 1920-21 he was President of the English Folk Lore Society and, from 1921 to the time of his death, of the Anthropological Institute. He held an Hon. L.L.D. of St. Andrews, and an Hon. D.Sc. of Manchester Universities. Not long before the end of his life his increasing interest in affairs had led him to accept an invitation to stand as a Labor Candidate for Parliament for the University of London.

He held his honours lightly. It is not of these that his friends think chiefly now; not of his learning; not of his avidity for work, or of his splendid loyalty to the claims of exact method; not of the laborious and remarkable accuracy of his earlier researches, or even of the brilliance of many of his later speculations. These all were great, and are having their profound influence upon many lives. But it is of Rivers as a man that we think; of his eager and unconquerable optimism, and of his belief in the possible greatness of all things human. Whatever may be the verdict of the years upon his published works, the influence of his vivid personality will remain for all who knew him as one of the best things that have ever entered their lives.

CALIBRATION OF THE GALTON WHISTLE

By Christian A. Ruckmick, Wellesley College

It does not seem to be generally known in this country that the tables which accompany Galton whistles made in Germany before 1914 have been found unreliable. Owing to the war the account of recent investigations carried on in Germany and the announcement of the change in calibrating the whistle have not come to the notice of many American psychologists and physicists. Yet the instrument is still widely used for testing the upper region of hearing in both normal and pathological subjects, and the change in the method of determining the pitch-values of the whistle in this region involves a difference from the former values of nearly an entire octave. It therefore seems important to gather together the facts bearing on this subject, and to review briefly the historical development of the whistle; for the improvements in the form of the instrument and revisions in the method of calibrating it have naturally gone hand in hand. There are certain characteristics that have won favor for the whistle in and out of the laboratory. It is compact and easily and quickly adjustable. Besides, it has the aspects of precision: it is set by means of a micrometer mechanism; it is capable of a double adjustment; and it is accompanied by a table of vibration-rates expressed to the last integer of a five-place figure and carefully written out in longhand by the manufacturers. Fortunately for decades researches have checked and rechecked the pitch-values, and recently the makers of the instrument have taken advantage of the available data in the construction of the whistle and in its calibration.

In the original form used by Galton the whistle was made "from a brass tube whose internal diameter was less than one-tenth of an inch in diameter. A plug was fitted into the lower end of the tube, which could be pulled out or pushed in as much as desired, thereby causing the length of the bore of the whistle to be varied at will."[1] The lip of the whistle was first the common lateral slit found in our toy whistles and similar to the lips of organ pipes and fifes. This was the type of instrument that Galton describes as having been used by him to test the hearing of animals in the Zoological Gardens of London. "It consists of one of my little whistles at the end of a walking-stick—that is, in reality, a long tube; it has a bit of india-rubber pipe under the handle, a sudden squeeze upon which forces a little air into the whistle and causes it to sound."[2] The next step in Galton's time was to attach the plug to a revolving cap which screwed it in and out and permitted it to be more conveniently and evenly adjusted. The degrees of revolution of the cap could now be marked on the circumference of the cylinder and the position of the plug could be noted to 1/250 inch. The corresponding pitch of the instrument was not empirically obtained, however, but was calculated directly from the length of the cham-

[1]Galton, F., Inquiries into Human Faculty and its Development, 1883, 38; *cf.* also 375. It was first described, apparently, in an article entitled "Whistles for Determining the Upper Limits of Audible Sound," S. Kensington Conferences (in connection with Loan Exhibition of Scientific Instruments), 1876, 61. See also *Nature*, xxvii, 1883, 491; Titchener, E. B., Experimental Psychology, 1905, II., ii., 31-46, *passim*; Sanford, E. C., A Course in Experimental Psychology, 1908, 379-381.

[2]*Inquiries*, 39.

ber, according to physical formula. Galton warns us against too great faith in the table of pitch-values when the chamber becomes very small and the lengthwise direction of the adjustment no longer follows the direction of the sound-waves inside the chamber. "When the tube is reduced to a shallow pan, a note may still be produced by it, but the note has reference rather to the diameter of the whistle than to its depth, being sometimes apparently unaltered by a further decrease of depth."[3] This place appears to be reached when the depth of the whistle is less than 1½ times the diameter or, for tubes of 1/10 in. diameter, at the setting of .16 inch, the equivalent of 14,000 d.v. It is for this reason that the instrument requires a small bore. The first forms were made for Galton by the surgical instrument maker, Hawksley, and by the optician, Tisley. Koenig also made instruments that were frequently used in early researches.

A marked change in construction came in 1894 when the late Max Th. Edelmann paid heed to suggestions from Bezold and made the lip adjustable. At the same time the cylindrical lip (locomotive-whistle type) took place of the the slit at the side: in other words, as we now have it, the whistle consists of two parts, the cylindrical lip from the pressure bulb, and the chamber into which the stream of air passes and from which the sound arises. The length of this chamber is adjustable as before, but a new micrometer arrangement changes the distance between the lip and the chamber. The last two are connected, of course, by a heavy bridge. There was also an intermediate form, of lighter construction and without the micrometer adjustment for the lip or the accompanying scale divisions.[4] In the most recent form, but one not widely used in this country, a single turn of the cap changes both the setting of the lip and the length of the chamber. The cap tapers so that, through a series of levers resting on its side, its forward movement into the chamber also controls the opening at the lip.[5]

More significant than these improvements in the form of the whistle is the progress made in the method of calibrating it. For a long time the Kundtian dust-figures obtained with lycopodium powder were extensively in use, but it now appears that they revealed the first overtone in each series of determinations and that to get this overtone the whistle was unwittingly overblown.[6] For this reason the upper audible tonal limit of

[3] *Inquiries.* 375.

[4] Edelmann, M. Th., Die Edelmannschen Grenzpfeifen, *Mitteilung No.* 10, München, 1914. Fig. 1.

[5] *Ibid.*, Fig. 4.

[6] A review of the investigations of the reliability of the Galton whistle which appeared prior to 1903 together with bibliography is given by Titchener, *op. cit.*, 32-36. The later researches which contributed to the new method of calibration are:

Schulze, F. A. Einige neue Methoden zur Bestimmung der Schwingungszahlen höchster hörbarer und unhörbarer Töne. Anwendung auf die Töne der Galtonpfeife und die Bestimmung der oberen Hörgrenze. *Ann. d. Physik*, 24, 1907, 785-822.

Debyre, P. Eine Bemerkung zu der Arbeit von Hrn. F. A. Schultze. *Ann. d. Physik*, 25, 1908, 819-823.

Hegener, T. Ueber die Tonbildung bei der Edelmannschen Galtonpfeife und deren Verwendung zur Bestimmung der physiologischen und pathologischen oberen Hörgrenze. *Beitr. z. Anat. Physiol. Pathol. u. Therap. d. Ohres. d. Nase u. d. Halses* (Passow u. Schaefer), 1, 1908, 321.

Helmholz, W. Vergleich zwischen der Galtonpfeife und dem Schulzeschen Monochord zur Bestimmung der oberen Hörgrenze. Diss., Berlin, 1910. pp. 18.

Winkelmann, A. Handbuch der Physik, 2nd ed., II, 1909, 210. The article briefly summarizes recent developments in the field of the upper limit of hearing.

the whistle was often given as high as the tone in the neighborhood of 50,000 d.v., while a number of recent studies point to a considerable reduction in this value.[7] The Edelmann Institute, which now makes most of the whistles, has consequently adopted an entirely different method of calibration, although it is a method proposed as long ago as 1897 by Stumpf and Meyer.[8] It consists, briefly, in the step-wise determination of critical points on a curve obtained through the production of difference-tones from several whistles and tuning forks in the presence of a sensitive (Schmidt) flame.[9]

The manufacturers have also made a decided improvement by inserting a diaphragm between the pressure-bulb and the whistle to prevent 'over-blowing'. This diaphragm has an aperture so small that under repeated tests it was impossible to produce an air-pressure greater than that of 100 mm. of water. On the other hand it is large enough to allow an adequate and even stream of air to pass. Through correspondence with the Edelmann Institute the writer has learned that Galton whistles may be returned to the Institute for recalibration at a very moderate expense. The whistle in the laboratory of Wellesley College was recalibrated during the summer of 1922 for the sum of $1.20; the charge for the new pressure-bulb with inserted diaphragm was 60c; and the postage was 12c. To avoid duty, the customs-tag should bear the notation that the instrument was originally purchased in Germany, is being returned for recalibration, and is educational in character. The address is: Prof. Dr. Max. Th. Edelmann & Sohn, Munich, Germany. Mr. C. H. Stoelting, 3037 Carroll Ave., Chicago, advises the writer that if the matter is handled through his firm he will undertake to register the tables in his records, thus making it comparatively easy to procure a copy of any table that might later become lost. The writer has also been informed that the older tables can not be translated mathematically into the present lower equivalents since each whistle has its own set of independent variables and must therefore be empirically calibrated.

The outcome of this revision is naturally important. All studies of the upper limit of hearing, of pitch discrimination, and of allied auditory phenomena in this region will bear review if they were undertaken on the basis of examination with the Galton whistle. For example, the highest

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[7]Of course this reading varied with the individual whistles. Edelmann, in Studien über die Erzeugung sehr hoher Töne vermittelst der Galtonpfeife (Grenzpfeife), *Ann. d. Phys.*, 4, 1900, 469-482, gives a setting (Tab. 1) for 54,900 d.v. and one (Tab. II; Galtonpfeife Nr. 331) for 48,000. Speaking of the older form he says: "Sie erzeugt....Töne, deren Schwingungszahl....für die älteren Constructionen bei etwa 25000 g.s. sichere Töne zu geben aufhört. Bei meiner neuen Form der Grenzpfeife geht der Ton bedeutend weiter in die Höhe und überschreitet für ausgezeichnet Hörende mit etwa 50000 g.s. die Hörgrenze" (*ibid.*, 469).

[8]A. Schwendt described the earlier procedure in two articles: Experimentelle Bestimmungen der Wellenlänge und Schwingungszahl höchster hörbarer Töne, *Pflüger's Archiv*, lxxv, 1899, 346, and Ergänzende Untersuchungen über Tonhöhenbestimmungen sehr hoher Töne mittels der Kundtschen Staubfiguren, *Verhandl. d. deutsch. otol. Ges.*, 1900, 55. Curiously enough, the method now adopted had been proposed previously to this time by C. Stumpf and M. Meyer, in Schwingungszahlbestimmungen bei sehr hohen Tönen, *Ann. d. Physik*, lxi, 1897, 760-779.

[9]Edelmann, *op. cit.*, 5f.

value given in the sample table in the Edelmann leaflet[10] for whistle No. 971 at 100 mm. of water pressure is 31,249 d.v.; above this point the whistle is declared to be unreliable. Our own whistle previously had a calibrated value of 45,000 d.v. at its highest point, but the new table gives it no setting above the value of 25,000 d.v. But in its latest improved form, with both a procedure of calibration and an actuating device that guard against 'over-blowing', the Galton whistle ought to return to service as a ready and dependable instrument in the determination of pitch sensitivity in the higher regions, of the upper limit of hearing, and of the many auditory defects and peculiarities that are presented in laboratories and clinics.

[10]Edelmann, *op. cit.*, 8.

POSITION vs. INTENSITY AS A DETERMINANT OF CLEARNESS

By KARL M. DALLENBACH, Cornell University

In all the experiments on the determination of attention that have been performed in the Cornell Laboratory,[1] spatial position has proved to be an important and specific factor. Meads, working with form *vs.* intensity, found that the intensity of the formed area (a Greek cross) varied greatly at the point of attentive equality accordingly as it was to the right or to the left of the 'formless' area.[2] Curtis and Foster, working with size *vs.* intensity, arranged their experiments so as among other things "to measure the error of space."[3] Their *O*s gave divergent results, but in one respect the results agreed: spatial position was found to be of more importance than an increase in area. Indeed, in the case of one *O*, "spatial position makes a greater difference in attention-compelling power than a fourfold increase in area."[4] In Bowman's repetition of Curtis and Foster's work the effect of position was likewise noted. Bowman found that "the left-hand position is the more favored."[5] Though the position-ratio varied greatly from *O* to *O*, the results justified the statements: "*the magnitude of the space error* proves that the effect of size was cut across and obscured by another influence,"[6] and "it seems that the left-hand position has an attentional advantage."[7]

It is the purpose of this study to compare in a quantitative way the 'attention-compelling' power of position and intensity, and to note to which of the four cardinal and four intermediate positions, if any, attention is predisposed. Two circular areas which varied only in position and intensity of illumination were compared, and the objective intensity at which the two were judged equally clear was determined for every one of the positions mentioned above.

Apparatus. The apparatus employed was that described in a recent number of this JOURNAL (34, 1923, 93 ff.) *O* sat in a light-proof booth with his head held firmly in position (in order to eliminate involuntary head-movements and to ensure constancy of position from experiment to

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[1] L. G. Meads, *Form vs. Intensity as a Determinant of Attention*, this JOURNAL, 26, 1915, 150-151; J. N. Curtis and W. S. Foster, *Size vs. Intensity as a Determinant of Attention*, *ibid.*, 28, 1917, 293-296; A. M. Bowman, *Size vs. Intensity as a Determinant of Attention*, *ibid.*, 31, 1920, 87-90.

[2] In Meads' published work (*op. cit.*) the effect of position is masked because he merely reported the general averages of his two spatial positions. His crude data, however, reveal that all his *O*s were predisposed toward one position.

[3] *Op. cit.*, 293.

[4] *Op. cit.*, 295.

[5] *Op. cit.*, 89.

[6] *Loc. cit.*

[7] *Op. cit.*, 90.

experiment) by a biting board which was so adjusted that the fixation point extended would fall midway between his two eyes. The fixation point was 1 m. away and 1 mm. in diam.[8]

The comparison areas, 8 cm. in diam. and centered on opposite sides and 14 cm. away from the fixation point, could be rotated around the fixation point so that the standard and the variable would appear now to the right, now to the left, below and above fixation, and also at the four positions intermediate between these. The positions of the areas varied as follows: in position 1, the variable was at the left of the fixation and the standard at the right; in position 2, the variable was at a 45°-angle below and to the left of the fixation and the standard at the same angle above and to the right; in position 3, the variable was below, the standard above; in position 4, the variable was at a 45°-angle below and to the right of fixation, the standard at the same angle above and to the left; in position 5, the variable was at the right, the standard at the left; in position 6, the variable was at a 45°-angle above and to the right of fixation, the standard at the same angle below and to the left; in position 7, the variable was above, the standard below; and in position 8, the variable was 45° above and to the left of fixation, the standard at the same angle below and to the right.

Exposure was made by illuminating the areas from behind. The duration of the exposure was controlled by an adaptation of Wundt's gravity tachistoscope. The apparatus was set for 1/10 sec., but the actual time used was, as measured by a Hipp chronoscope, $100.3 \pm 0.7\sigma$. The light was derived from a 100-watt concentrated-filament Mazda lamp placed in a projection lantern behind the tachistoscope. The rays of light when uninterrupted by the tachistoscope passed through a light-proof funnel to the posterior frame of a diffusion chamber and thence, through circular windows, and through the connecting tubes of the chamber, to the ground-glass windows in *O*'s booth. The intensity of the illumination was controlled by varying the amount of light that was allowed to pass through the connecting tubes. The full amount of light that would pass through the window in the posterior frame, which was 8 cm. in diam., was taken as .100% or unity. As the diameter of this opening was varied by the insertion of different metal shutters the amount of light falling upon the anterior windows, those viewed by *O*, would also vary. The circular openings were placed in the center of the shutters so that difusion should be equal in all directions, and they were made of a size to decrease the amount of light allowed to pass by steps of 4% from 100%-40%, by steps of 2% from 40%-20%, by steps of 1% from 20%-1%, and by steps following the negative power of 2 from 1%-1/32%. The intensity of the standard was taken at 16%, and of the variable within the limits just stated.

[8]Both *O*s reported that the fixation-point was not always stationary, that it at times moved about the field of vision. For the most part it appeared in its proper position, direct in line with the axis of the Cyclopean eye, but at times it seemed as if it was displaced "as much as a foot" from the true position. When the shift was observed the point usually appeared as given in the new position, but at times movement was itself perceived. The movement in such cases was described as slow and unsteady, drifting and oscillating like the bobbing of a cork upon a gently moving stream. Too few of these shifts and movements, which of course were subjective (neither the fixation-point nor *O*'s head, because of the biting-board, could move from the fixed position), were reported to justify any statement regarding a central tendency. Suffice it to note here that neither the direction nor the duration, insofar as our data warrant any statement, was constant: the direction varied to all points of the field, and the duration was at times short, at other times long.

Observers. The *O*s were Mrs. A. K. Whitchurch (*W*), a graduate student in psychology, and the author (*D*).[9] Both were highly practised in the observation of attributive clearness. *W* worked without specific knowledge of the problem. The *O*s served three times a week and always at the same hour of the day. The period of observation was approximately an hour in length. A rest period was offered whenever the *O*s demanded it, but at least one rest was taken during every hour.

Procedure. The following instructions were given to the *O*s: "At the signal 'Ready' fixate the spot of light before you; at 'Now' two areas of light will be shown; report which, if either, is the more clear. Give the position of the clearer as 'right', 'left', or 'above', below', whichever the case may be; if they are equally clear say 'same'; if you are uncertain, report 'doubtful.'" The experiments reported as 'doubtful' were immediately repeated. Though few in number such reports were given by each *O*.

A series of preliminary experiments was conducted by the method of limits to determine for each *O*, and for every one of the eight positions, the approximate intensity of the variable necessary for equality of clearness. Upon the basis of these experiments 5 stimulus-values were selected for each *O* for every position of the variable, and the main experiments were conducted on the principle of the method of constant stimulus-differences. The number of stimuli was later increased to 7 for *W* in positions 3 and 4, since it was found in both these cases that the values first selected were too low.

Twenty observations were taken at every position for every value of stimulus. At the beginning of every experimental hour a short warming up series was given. The procedure in the regular series was different for each *O*. *D* completed the experiments at one position before passing to the next. He always knew where to expect the stimuli. The positions were given him in the order 1, 2, 3,....8. *W* on the other hand never knew what position the stimuli would occupy. The positions as well as the stimulus-values were given her in haphazard order.

Results. The results of the experiment are shown in the accompanying table, which gives for each *O* and for every position of the stimuli the per cent. of illumination necessary for the variable to be reported equal to the standard in attributive clearness.

TABLE I

Showing for Each Observer and for Every Position of the Stimuli the Per-cent. of Illumination of the Variable Necessary for the Report of Equality in Clearness.

POSITION	OBSERVER			
	D		W	
	% ILLUMINATION	PRECISION (h)	% ILLUMINATION	PRECISION (h)
1	9.9168	.0989	6.0989	.1540
2	5.0984	.1999	3.6399	.3233
3	20.5932	.0379	39.6936	.0723
4	18.8796	.0552	41.6792	.0697
5	28.0568	.0276	48.7870	.0434
6	44.6260	.0231	56.8570	.0540
7	10.1760	.0882	6.0086	.2242
8	15.6680	.0419	6.6383	.2059

[9]My thanks are due to Miss Doris B. Dewey for serving as *E* while I acted as *O*.

These data, in spite of a large individual variation, show that position is for both *Os* a condition of clearness. The areas to the left and above the point of fixation (positions, 1, 2, 7 and 8) require less illumination to equal the standard in clearness than those below and to the right (positions 3, 4, 5 and 6). The most favorable position for both *Os* is position 2, the area to the left and 45° below fixation; and the most unfavorable is position 6, which is the opposite, to the right and 45° above fixation. In the first case the percent. of illumination necessary for the variable to equal the standard in clearness is 3.6% and 5.1% for *W* and *D* respectively, and in the second case it is 56.8% and 44.6% respectively.

The data show, furthermore, that position was more effective for *W* than for *D*. All *W*'s values are without exception more extreme than *D*'s. Whether this result is due to individual difference or to difference in the mode of procedure followed in the two cases it is impossible from the facts at hand to say. It is certain, as the results of the experiments mentioned above clearly show, that large individual differences obtain in these investigations; but the fact must not be lost sight of that *D* knew for every experiment where the stimuli would apear, and this knowledge may, in part at least, have been responsible for the difference. Whatever the effect of *D*'s knowledge may have been, we feel, in the light of the results, that the haphazard method of varying the positions is preferable.

Considering the difference of procedure followed with the two *Os*, the uniformity of their results is very marked. The data vary in the same direction and the rank-order of the positions in their 'effectiveness' is extremely close. The order for *W* is 2, 7, 1, 8, 3, 4, 5, 6; and for *D*, 2, 1, 7, 8, 4, 3, 5, 6. A correlation, as figured by Pearson's Method of Rank Differences, of .95 exists between the results.

The uniformity of the results leads us, in spite of the fact that they were obtained under special conditions and with but two *Os*, to conclude that position is a condition of clearness, and that the left-hand position and the position above have a distinct attentional advantage over the right-hand position and the position below.

Titchener, in his exposition of the conditions of clearness,[10] says that they "are of a kind to produce a powerful effect upon the nervous system," and that they have "biological significance." It is therefore perhaps worth noting that position, on both of these counts, takes its place with the other conditions of clearness. From the neurological point of view the effectiveness of position may be explained by the integration of the neural connections. The processes which lead from the rods and cones of the right retinas (those excited by stimuli to the left) center in the right hemisphere of the brain and there arouse excitations which, because right-handed people are left brained, are not interfered with by other excitations. Both of our *Os* were right-handed; so that this explanation would have added plausibility if it should prove, by extension of this experiment, that the right-hand position has an attentional advantage for left-handed people. From a biological point of view the most probable source of danger for primitive man was from above. The early reptiles and other gigantic beasts of prey struck down. This would account for the attentional advantage of the position above. Continuing this imaginative trend of thought, the advantage of the left may thus be explained: when man became man's greatest danger it was the weapon held in the antagonist's right-hand, and therefore appearing at the left, which demanded attention. Speculation, of course! But the admission of biological speculation merely places position once more in line with the other conditions of clearness.

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[10] *Textbook of Psychology*, 266 ff.

The results of this experiment corroborate those of Meads, Curtis and Foster, and Bowman, and show that position cuts across form and size because it is itself a condition of clearness. The conclusion that position is a condition of clearness also confirms that made by the author, in his study of "Attributive *vs.* Cognitive Clearness," "that certain areas are favorable for clearness".[11] Moreover, the positional disposition of clearness agrees closely with the perceptual distribution of attention, and it may well be that the latter is conditioned and determined by the former.

Summary. The results of this study may be summarized as follows: (1) position is a condition of attributive clearness; (2) the left-hand position and the position above have an attentional advantage; and (3) the most advantageous position of all is the position to the left and below the point of fixation.

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[11] *Jour. of Exp. Psych.*, 3, 1920, 183-230; esp. 205, 213, and 229.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LXIII. The Phenomenological Description of Musical Intervals

By E. M. Edmonds and M. E. Smith

In this study we have sought to discover whether the experience resulting from the simultaneous presentation of two tones of different pitch can phenomenologically be distinguished from other similar experiences; and, in that event, whether a qualitative term can be found that shall adequately characterize it. Former experiments with similar processes have been directed towards their relative characterization by, *e.g.*, degree of fusion, of consonance, of pleasantness, of confluence, of smoothness, and the like.[1] In this investigation, however, we have been concerned with the possibility of an absolute characterization, a phenomenological description of the bitonal process in and for itself.

In preliminary experiments we employed as tonal material an harmonium, and we limited ourselves to the major and perfect intervals of the one octave c'—c''. The *Os* were two graduate students in psychology, *M* and *A*, both of whom were musically trained and knew intervals by their musical names; two members of the instructing staff, *H* and *Bi*, neither of whom had had musical training; and an undergraduate student in psychology, *Ba*, who played instruments by ear but had no technical knowledge of music. Our formal instructions to the *Os* were framed after preliminary experiments, in which we found that, although a phenomenological difference between intervals was immediately apparent, the *Os* had ready no words to characterize this difference, and further that the task was much easier of fulfilment if two intervals, the one of which served to 'set off' the other, were given successively. Our first instructions ran, therefore, as follows: "I shall give you in succession two tonal fusions. You are to try to find adjectives which shall characterize or describe them. You may select a word from the list given below, or you may furnish some other term which for you is more descriptive."[2] Our procedure, consequently, was to present every interval with every other as in the method of paired comparisons. In a single experiment each interval was sounded for 4 sec. with an interval between the pair of 3 sec. *O* sat where he could not see the keyboard, and upon the conclusion of the second interval characterized both intervals as immediately as possible. If *O* requested it, the two intervals were repeated.

Results of Preliminary Experiments. All *Os* had difficulty at first either in understanding the instructions or in assuming the phenomenological attitude. Eventually, however, after repeated trials, 4 of the 5 succeeded

[1]For the most recent study of this kind and for references to the earlier literature see C. C. Pratt, Some Qualitative Aspects of Bitonal Complexes, this Journal, 32, 1921, 450.

[2]The list, which was typewritten and placed in the hands of the *O*, was intended merely to be suggestive; the terms were chosen by free association from words descriptive of similar patterns in other sense departments: smooth, rough (coarse), hollow (dilute), harsh, sweet, rich, astringent, pungent, gritty, sparkling, lustrous, radiant, and sombre. If necessary, the *O* was warned against the employment of aesthetic or affective terms.

not only in finding terms that characterized every one of the 8 intervals, but also in recognizing by means of its quality any one of the intervals when presented in chance order. The fifth *O*, *H*, was at times able to find characteristic terms, but failed completely as regards recognition.

The terms ultimately decided upon as best descriptive of every interval by every one of the 4 *O*s were as follows. *Octave*: *A*, "smooth, like the surface of window glass;" *Ba*, "smooth, like polished steel;" *Bi*, "smooth, a unitary experience like one note;" *M*, "smooth, like the feel of ice-cream in the mouth, or the touch of polished glass." *Major Seventh*: *A*, "astringent, like the taste of a persimmon;" *Ba*, "gritty, like the feel of small sharp granular objects;" *Bi*, "astringent, like strong vinegar or alum;" *M*, "harsh, a nippy, biting effect like a strong astringent." *Major Sixth*: *A*, "sweet, satisfying, with no sustenance;" *Ba*, "astringent, intermittent puckeriness associated with roughness;"[3] *Bi*, "rich, a lusciousness;" *H*, "luscious, like a juicy mellowness;" *M*, "sweet, like the flavor of fruit, softly rich in the sense of flavor." *Perfect Fifth*: *A*, "hollow, like a hollow tree, no body to it;" *Ba*, "hollow;" *Bi*, "hollow, empty, has no body;" *M*, "hollow, like a hollow tree or a cave," "like a sound produced by tapping on a hollow wooden box." *Perfect Fourth*: *A*, "rough, like the feel of coarse sandpaper;" *Ba*, "harsh, like the feel of a coarse, granular surface;" *Bi*, "harsh, with a little astringency, thick and heavy;" *M*, "a certain coarseness, rich, oily, hard, lumpy." *Major Third*: *A*, "mellow, like biting into a very ripe apple;" *Ba*, "thick, like bean soup;" *Bi*, "luscious, like peaches to the taste;" *M*, "mellow, like sweet fruit or a soft diffused light." *Major Second*; *A*, "gritty, like the feel of small pebbles in the hand or grapenuts in the mouth;" *Ba*, "rough;" *Bi*, "gritty, like sand in one's teeth;" *M*, "gritty, like sandpaper."

At this stage of the investigation it became apparent that the intervals within the octave have in fact a perceptive 'quality' which our *O*s could roughly characterize, that there was fair agreement among the *O*s as regards the nature of this 'quality', and that the interval could be instantly recognized in terms of its 'quality.' Trial experiments made with the harmonium in other octaves seemed to indicate that the character of the interval was much the same elsewhere. We thought it advisable, however, to continue the experiments with relatively pure tones, and to increase, as far as our materials would allow, the range of frequencies. We shall, therefore, postpone discussion of the results of the preliminary experiments until we have described our further experiments.

Results of Principal Experiments. In this second group of experiments we used a set of König forks mounted on resonance boxes. The frequency of the forks ranged from Ut^3 (512 vs.) to Re^5 (2304 vs.).[4] In the first octave, Ut^3–Ut^4, we had forks which gave all the harmonic intervals, and in the next octave we had Mi^4 (1280 vs.), Sol^4 (1536 vs.) and Ut^5 (2048 vs.). By combining these forks in various ways we were able, therefore, to obtain 4 intervals of the octave, 2 of the major seventh, 2 of the major sixth, 7 of the perfect fifth, 5 of the perfect fourth, 4 of the major third, and 6 of the major second. Of these 30 intervals 21 were true harmonics, 6 failed of being true by 1/10 of a vibration or less, and of the remaining 3 we discarded a sixth (Ra^3–La^3) and a second (Ra^3–Mi^3). Another interval of the second (Sol^3–La^3), which we retained, failed of being a true ratio by 10.7 vs., but no difference in 'quality' with respect to other seconds was reported.

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[3]Both the sixth and the seventh, especially when given by the harmonium, arouse in some *O*s a muscular reaction which is characterized as a "twist or contortion of the body", "a wry face." *Ba* was apparently unable to dissociate this muscular reaction from the quality of the interval.

[4]These are, of course, single vibrations.

The apparatus designed for the simultaneous actuation of a pair of forks consisted of a platform set between two iron supports upon which an axis rotated. Two felt piano-hammers were clamped to this axis in such fashion that they could be adjusted to strike the two forks by means of a quick rotation of the axis. Across each end of every resonance box on the bottom was glued a strip of saddler's felt ½ in. thick; the platform was covered with coarse sandpaper. When, therefore, the forks on their resonance boxes were placed on the platform directly in front of the two hammers, the felt strips served not only to prevent secondary resonance but also, by friction with the sandpaper, to keep the forks in position when struck by the hammers. Care was taken so to adjust the hammers that the forks were struck simultaneously, with equal intensity, and with the avoidance, so far as possible, of partial tones.

The *Os* were *A*, *Bi*, and *M*, of the previous experiment. To these was added *W*, a member of the instructing staff who had full knowledge of the investigation. The instructions were: "I shall present to you a tonal experience and I want you to characterize this experience as best you can with a single word. We give below a list of terms as suggestive. You may take any one of these if it characterizes the experience, or you may substitute one that seems to be more accurately descriptive." The procedure was as follows: after a 'ready' signal the forks were actuated, and before either tone had died away they were restruck, until *O* had given a judgment which he thought was satisfactory. The forks were then damped, *O*'s report was written, and a new interval was arranged. The stimuli were presented in 3 groups of 5 series each. In Group 1 every one of the 5 series was presented in the order second, octave, seventh, fourth, sixth, fifth, and third. In Group 2 the intervals were chosen at random, and in both groups all intervals were within the range of the octave (Ut^3—Ut^4). In Group 3 the serial order of intervals was the same as in Group 1; the intervals, however, were within the ninth (Ut^4–Re^5).

The characterization of the various intervals was, on the whole, the same as in the harmonium experiments. In general there was an increase in the number of analogies and in particular of analogies of the taste-touch order. The *octave* is still described as "smooth, like the feeling of ice-cream in the mouth;" "smooth, like molasses in the mouth;" "a smooth feel in the mouth, but I have no likeness for it." The *seventh* is, as hitherto, said to be astringent by all *Os*; *M*, however, found "no definite likeness in the mouth;" it was rather "harsh or blurred, like the feel of harsh fur;" and *A* similarly reports "dilute, sour, a little rough; a soft roughness, like a lamb's wool powder puff, with some body to it." The *sixth* is again characterized by all *Os* as "luscious, succulent, rich, like rich pudding-sauce in the mouth; sweet thick, like sugar syrup." The *fifth* proved to be the most difficult of all intervals, particularly as regards its differentiation from the fourth. It was described as either "thin, dilute, like a clear soup," or "hollow, like a hollow space," or again as "rough or coarse rough, like the feel of tweed suiting," or "hollow rough, like the sound of a husky voice." The *fourth*, on the other hand, was said to be "rich, like the richness of cloth texture, velvety;" "rich, like polished ebony, shining, but dark;" "rich, like rich, crumbly cake in the mouth;" or "lustrous, like shining metal;" or again, "harsh, like a ball of crushed paper in the hands"; "harsh rich, like the feel of cotton wool in the hands"; "rough, like coarse sandpaper to the hands." The *third* is uniformly described as "mellow, like ripe fruit;" less frequently, as "sweet, like hard sweet peppermint candy in the mouth;" it is similar in character to the sixth, but not as "thin," as "juicy," as the sixth. The *second* is in general "gritty, like the feel of wheat in the fingers, or dry corn flakes in the mouth;" or again, "pebbly, like round pebbles in the hand;" or "grating, like the feel of a nutmeg grater or the feel of sandy spinach in the mouth;" occasionally it is confused with the seventh as astringent.

At the close of the three regular groups we gave a comparison series in which the *Os* were presented first with an interval in a lower octave and then with the same interval in the higher octave, and were asked to indicate any difference, if it occurred, between the qualities of the two intervals. As a result of these experiments, no change was found in the smoothness of the *octave*; the higher interval was said, however, to be more dilute, brilliant and clear. The astringency of the *seventh* was unchanged, the upper seventh differing only in body or volume. No change was found, again, in the lusciousness of the *sixth*; it was described, however, as brighter in the higher than in the lower octave and also as having less volume. The hollowness, coarseness, harshness of the *fifth* was unchanged. The upper interval, however, had less body and warmth than the lower. The roughness of the *fourth* and of the *second* was in both cases finer in quality; the higher interval of the second, for example, was said to be a finer grit than the first, the pebbles were sharper to the feel. The mellowness of the *third* was unchanged. It was, however, said to be smoother in the upper than in the lower interval.

We may, therefore, summarize the characteristic qualities of these intervals as follows: the octave smooth; the seventh astringent, sharp-rough, hollow-astringent; the sixth luscious, juicy-mellow, succulent; the fifth dilute, hollow, harsh; the fourth rich, harsh, coarse; the third mellow, sweet (to taste); the second gritty, grating.

It will be observed that the terms employed belong to three different categories. We may, first of all, characterize the various intervals by reference to taste-touch fusions. Taking the intervals in order from octave to second they would be smooth, like smooth ice-cream or smooth molasses; astringent, like the feel of a green persimmon; luscious, like a juicy, succulent fruit; dilute, like a clear soup; rich, like whipped cream or like ripe canteloupe; mellow, like a ripe but not juicy fruit; gritty, like corn flakes or other coarse substance in the mouth. Or we may characterize them in terms of touch fusions, in which case we should have, taking the intervals in the same order, smooth, like polished glass; harsh, like fine sandpaper; no term for the sixth; coarse, like tweed suiting; harsh, like crushed paper or crash; no term for the third; gritty, pebbly, like pebbles in the fingers. Finally, the interval of the fifth was most frequently described as hollow. We believe that this term had reference to the sound of the experience. It is the only descriptive term in our series which belongs to an auditory category.

Attitude. At the close of our experiments we wished to characterize the attitude under which the above observations were made. It will be recalled that at the outset our *Os* had difficulty in fulfilling the requirements of the instructions and also that one *O*, *H*, was unable to describe the intervals with any degree of consistency. These facts point to a characteristic attitude for the observation, and we desired to know, if possible, what the attitude was. Accordingly, we presented two series of intervals to every *O* with the instructions to listen to the experience and to report the descriptive term as before, and then to state as best he could what he actually did in making the report. Typical statements of the *Os* are as follows. *A* says: "the experience came as a whole, as one bit of experience, and the term gritty came at once with a gritty feeling in my mouth;" "heard as a whole, got feeling of ripe peach in my mouth, mellow is a quick judgment;" "got two tones coming together into fusion, lost completely the feeling of mellowness." *M* reports: "the sound comes as a single experience, then I get roughness;" "unitariness of experience impressed me first, I didn't hear out the two tones until after the judgment;" "experience came as one thing, musical association came next; I repressed this to get psychological attitude, then got judgment of lusciousness;" "I got total experience, then doubt as to term to use, next I heard out the two tones, filled in a third between, and recognized the interval as the fifth, did not get judgment of

characteristic quality." *Bi* reports "one piece of experience, a mass of tonal experience, lumpy, pebbly;" "came as unity, with a feeling of a sheet of smooth paper, so unified that after judgment I heard out the two tones to make sure they were both there;" "unitary experience, buzzing and harsh, like hand going over coarse cloth;" "mellowness came first with unitary experience before realizing that two tones were present; I don't get good mellowness unless the two tones are equal in intensity; if I hear out the two tones the mellowness goes; analysis robs it of its mellow quality." It will be noticed that the three *Os* agree that the sound comes as a unitary whole, that its 'quality' seems somehow to inhere in it, *i.e.*, the sound comes as mellow, as smooth, as lumpy, etc., and that any attempt to analyze the total experience into its components changes the nature of the experience in the sense that its 'quality' disappears. The reports of *H* in the harmonium experiments show that his attitude was of the analytic sort. For example: "upper note stands out, but the beating tone is very close to the upper in pitch and fuses more or less with it;" "this interval is very uniform in all its dimensions, it goes straight on, it is rather small;" "small in volume; two tones, very much alike in pitch; within the complex is a very rapid beating tone, which is rough;" "almost complete integration of the tones, although both tones are there; this integrative tone increases and decreases at a slow rate; it may be an alternation in volume, or perhaps in intensity;" "rapid beating tone that lies pretty close to the upper primary, but is not the same in pitch; the upper tends to fuse with the beating tone, giving it body." Since, therefore, in these experiments the phenomenological characterization required, not an analysis of the complex experience, but rather the taking of it as a whole, the reason for *H*'s difficulty is apparent.

Conclusions.—Our conclusions cannot, of course, be more than tentative. We have found, however, that it is fairly easy to fit to bitonal fusions characteristic 'qualitative' names, derived from taste and touch, which make recognition of the fusions possible. There is much closer agreement among *Os* than we had anticipated. The names are overtly analogical, but they perhaps represent the sort of characterising names that ordinary language would have imposed if the fusions in question were natural and common parts of our everyday perceptive experience.

LXIV. Thunberg's Illusion

By Ida Bershansky

Thunberg, in a discussion of cutaneous complexes, remarks that a cold sensation which is not accompanied by a sensation of contact is frequently apprehended as caused by a liquid. This interpretation may, he thinks, be explained by the fact that, when a part of the body is submerged in a liquid, the pressure sense remains practically unexcited, so that the temperature sensations aroused are approximately unmixed with pressure. In this connection he describes an illusion of wetness which, he says, appears when the forehead is stimulated with a very cold substance for an interval of 20 sec. After the removal of the stimulus the cold continues for some time, during which the skin of the forehead is distinctly sensed as wet.[1]

These statements are opposed to results obtained in other experiments. Tung, for instance, points out that Helmholtz and later Bentley declare

[1]T. Thunberg, in Nagel's *Handbuch der Physiologie des Menschen*, III, 1905, 708. The original passage is quoted by S. Tung, this Journal, 32, 1921, 421.

that wetness is occasioned by a simultaneous stimulation of cold and pressure, and Tung himself obtained the experience of wetness by simultaneous stimulation of a cold and a pressure spot. We have thought it advisable to make a systematic study of Thunberg's illusion, in the hope that we may not only remove the discrepancy, but also describe the perception of wetness itself more definitely than Tung, under the conditions of his experiment, was able to do.

Our experiment falls into three parts. The plan of the first was in an initial series to study the course of the pressure experience under Thunberg's conditions, but with temperature reduced to a minimum, and in particular to see whether any after-sensations of pressure might appear in the period immediately following the removal of the stimulus; and then, in a second series, to admit a cold with the pressure stimulus and in the same way to study the course of the resulting experience. Since Thunberg gives no hint of the intensity of the touch stimulus, we employed stimuli of 6 different weights; these consisted of circular discs of brass of approximately 1, 2, 3, 5, 10, and 25 gr. in weight. The diameters of the first two were 1.3 cm., and of the remaining discs 1.9 cm. Since, also, we desired to use the same stimulus objects in both series of this group of experiments, we attempted in the first series to reduce the cold quality of the metal by (*a*) covering the face of every disc with felt, (*b*) warming the discs by holding them in the closed hand, and (*c*) warming them slightly in the rays of an electric heater. In the second series the discs, with felt removed, were chilled by placing them in a bath of cold water, the temperature of which ranged from 16° to 19° C. In order to make the stimuli easier of manipulation and more uniform of application, and at the same time to eliminate any arousal of warmth by the hand of *E*, the weights were lifted by means of a cord, 10 cm. in length, which was inserted and wedged in a small hole drilled in the center of every disk.

O reclined comfortably in a Morris chair with the head thrown back so that the forehead was in a horizontal position. Preliminary trials showed that, if the stimulus objects were placed near the eyebrows, eye-movements were set up which interfered with the course of the pressure experience; we therefore stimulated the upper forehead as near to the roots of the hair as was possible without exciting them. The *O*s in this group of experiments were *B* and *W*, two members of the instructing staff, and *M*, an undergraduate specializing in psychology. In order to shorten the period of report we used two instructions. The first limited the report from the time the stimulus was removed until all after-sensations disappeared, and read as follows: "I shall give you a cutaneous stimulus on the forehead, and then, during the course of the resulting experience, I shall give a signal 'now.' You are to report the qualitative and intensive course of the experience which ensues after the signal." The second instruction by appropriate signals restricted the report to the interval between the moment of stimulation and 10 sec. after the stimulus was removed. *E* sat behind *O*, and every stimulus of each series was presented 8 times over in chance order.

Results of the First Part of the Experiment. In describing the results of these experiments we shall combine the reports received under the two instructions and, further, since there was a typical difference in the course of the pressure experience resulting from stimulation with the discs of 1, 2, 3, and 5 gr. on the one hand, and of 10 and 25 on the other hand, we shall refer to these groups as the lighter and heavier stimuli. With the lighter stimuli the course of the "pressure" experience was in general described as (1) a light pressure or tickle, (2) a weak, filmy cold or "cool dampness", (3) a weak pressure which either faded out or else continued until the stimulus was removed, and (4), after the removal of the stimulus, either "nothing", or a faint contact or tickle, which gradually disappeared. With the heavier stimuli the course was (1) an intensive pressure, (2) a cold, wet pressure, or a continued pressure, (3) wet, and (4), after the removal of the

stimulus, an areal pressure which either gradually died out or else fluctuated in intensity until it finally disappeared. Occasionally, however, immediately after the removal of the stimulus, there was a short interval before the appearance of the pressure when "nothing" was felt. It will be observed that with both groups of stimuli coolness, cold, dampness, and wetness were reported, despite our efforts to eliminate the temperature factor; dampness appeared only with the lighter stimuli, wetness with the heavier. It will also be noticed that after the removal of the stimulus dampness or wetness never appeared; that the after-pressure with the lighter stimuli was very weak; and that with both groups of stimuli there were instances when no after-sensation was observed during a brief interval following the removal of the stimulus.

With the cold stimuli, the course of the experience following upon the lighter stimuli was exceedingly variable. In general, however, we may say that the course was (1) pressure, (2) cold, (3) cold damp, (4), after removal of the stimulus, either nothing at all or light pressure, or again faint coolness, occasionally a faint, cool damp, and (5) cool dampness or faint pressure that gradually faded out, sometimes alternating with a faint coolness or cool dampness. Except with the 1 gr. stimulus, when no after-sensations of any character are sometimes reported, the 'empty' interval is usually followed by cool damp or by faint pressure. With the heavier stimuli the reports were less variable. The most frequent course was (1) cold pressure, (2) wet, cold wet, or cold which continued until the stimulus was removed, then (3) cold pressure or wet, which gradually became less in intensity, sometimes alternating with cool or pressure and as a rule ending with a faint pressure or tickle.

The outstanding result of the investigation thus far is the exceeding variability of the reports. Although there is abundant evidence of the presence of an after-sensation of pressure in the period following upon the removal of the stimulus, which would seem to bring the 'illusion' into line with other investigations, it is plain that we have not obtained the compulsory conditions, if there are any, for the perception of wetness; and, furthermore, we have insufficient data as regards the nature of the integration. We decided, therefore, at this point, to continue the investigation with other *Os*; and, since the lighter group of stimuli gave the most variable results, to discard them and employ in the future only the heavier stimuli.

Results of the Second Part of the Experiment. In the second part of our investigation the stimuli were three in number and were of the same material, shape, and diameter (1.9 cm.) as before; they weighed 20, 25, and 30 gr. The *Os* were *G* and *H*, instructors in psychology, and *Br*, a graduate student in psychology. The procedure was the same as in the earlier experiments, with the exception that a mechanical instead of the manual method of applying and removing the stimulus was employed. The instructions were unchanged. For the cold-pressure series the stimuli were chilled in ice water, the temperature of which ranged from 11° to 15° C. The experiments were performed in July and August, when the coolness of the metal gave us less trouble in the pressure series; temperature was practically eliminated by the felt cushion which we employed in the earlier experiments.

In the pressure series the course of the experience was practically the same for all *Os*. It began with (1) a neutral pressure which changed to (2) a sub-cutaneous quality (drag or ache), and then for two of the three *Os* was frequently tinged with (3) a slight coolness. Following the removal of the stimulus there was (4) a sudden drop in intensity of the drag or ache quality, and then (5) an uniform decrease in intensity and change in quality from a subcutaneous to a cutaneous pressure.

In the cold-pressure series the typical course was as follows: (1) fairly intensive pressure, (2) cold or cold wet, and (3), if (2) were cold, cold wet

until the stimulus is removed. Immediately upon the removal of the stimulus the wetness disappears and either a pressure (2 *Os*) or a cold pressure (1 *O*) follows. Thereupon (4) the course is either (*a*) pressure, cool wet, cool; or (*b*) pressure, cold, wet, cool; or again (*c*) cool pressure, wet, cool. It will be observed that the qualitative course after the removal of the stimulus is with one exception the same as that during the period of stimulation. The difference between the two periods is a difference in intensity and in the final quality. The fact that the end-stage of these experiments is cool, whereas the final stage of the previous experiments was usually pressure, is probably conditioned upon the greater coolness of the stimulus in these experiments. It will be seen also that wetness is almost invariably preceded by a pressure which changes to cold or else by a cold pressure, and that ultimately wetness breaks up either into cool or, as in the earlier experiments, into pressure.

Results of the Third Part of the Experiment. Before discussing the results as a whole we wish to report the third part of our investigation, which was concerned with the appearance of the illusion on other parts of the body than the forehead. It will be recalled that we began by stimulating the cutaneous surface of the forehead, because that locality was one of Thunberg's conditions; but a systematic study of the 'illusion' such as we planned would be incomplete without reference to other parts of the cutaneous surface.

We employed the same stimuli, the same procedure and the same instructions as in the second part of the experiment. The *Os* were *Br*, who had served in the previous experiment, and *S*, an undergraduate student specializing in psychology. The parts selected for investigation were the forearm, the leg just above the knee, and the middle of the back.

In general it may be said that the illusion occurs on the four parts of the body investigated with approximately equal frequency. The average percentage of occurrence in these experiments for the two *Os* was: on the forearm 87.5, on the leg 94, on the back 95. The experience, however, was reported by both *Os* as more vivid on the forearm and back than on the leg; the pressure, in particular, was less intensive and adapted more quickly. As regards the general course of the experience the results of these experiments merely substantiate those of the earlier experiments, and we have therefore nothing new to report.

Discussion. We now turn to a discussion of the two problems with which we originally set out; first, an explanation of the Thunberg illusion in terms of other experiments of a similar nature, and secondly, a more definite description of the perception of wetness. As regards the former, it seems clear from our results that the illusion as described by Thunberg is an integration of after-sensations of cold and pressure. In support of this interpretation we have the following evidence. (1) In the pressure series with the heavier stimuli after-sensations of pressure are almost invariably reported immediately upon the removal of the stimulus. (2) In the cold-pressure series with the heavier stimuli either pressure or cold pressure is again almost invariably reported before wetness. (3) With weak stimuli pressure is as frequently reported immediately upon the removal of the stimulus as 'faint coolness,' and both more frequently than damp. (4) There is a decided correlation on the one hand between weak intensity of stimulus and dampness, and on the other hand between strong intensity of stimulus and wetness. In other words, the difference between dampness and wetness seems to be referable primarily to a difference in degree of intensity of pressure. The reason for Thunberg's failure to observe the after-sensation of pressure will, we think, be apparent when we have discussed the nature of the experience of wetness.

This experience may perhaps be best described in the words of *H.* "Psychologically wet is a compact cold; whenever wet becomes less compact, wetness disappears and, in general, a diffuse cold is left." It is, apparently, the pressure quality which gives the compactness to the cold, and under a psychological attitude wetness may usually be analyzed into cold and pressure. When, however, the cold is exceptionally intensive, it is exceedingly difficult to obtain a clear experience of pressure; and since, as Tung observed, the perception of wetness requires the greater clearness of cold, it is not strange that the pressure should, in Thunberg's observations, have been overlooked. Wetness itself is a meaning put upon the integration of cold and pressure; and as a rule this meaning is associated with some object; *e.g.*, "a wet which moved on my forehead," "a fleeting wet which ran down the forehead," "it is like a drop of water." Dampness, on the other hand, is psychologically a less intensive pressure and perhaps also a less intensive cold; the integration is a less compact coolness, and the object-meaning of dampness is, accordingly, "a light breath of damp air," "being touched lightly by a wet cloth," "like the brush of a slightly wet feather."

Summary. The Thunberg illusion occurs with approximately equal frequency on the forehead, forearm, leg and back.

Wetness is a perception which psychologically is an integration of pressure and cold, the latter having the greater intensity and clearness.

The 'illusion' of wetness is conditioned upon after-sensations of pressure and of cold.

SOME RECENT PSYCHOANALYTIC LITERATURE

By G. Stanley Hall

The New Psychology and the Teacher. By H. Crichton Miller. N. Y., Seltzer, 1922. pp. 225.

The purpose of this book is to "help the teacher to gain something of the analytical point of view and having done that to draw his own conclusions." The author does not restrict himself, however, to psycho-analysis, but covers a certain portion of the older psychology. He deplores the divergences of the Vienna and the Zürich school, finding that almost diametrically opposite philosophies underlie them, Freud being a thorough-going determinist while the doctrine of free-will is implicit in all of Jung's work. These differences justify a detached and critical attitude, and it is a matter of regret, though not of surprise, that this justification is not recognized by the founders of either school, and that they follow the example of most pioneers in resisting compromise and criticism alike.

Educationists above all need freedom of criticism, for their interest is focussed at a point where many paths meet: art and philosophy; body and mind; memory and imagination; science and religion,—to name only a few of these paths. There is no panacea or master-key, and this book is simply the contribution of a physician who would not be dogmatic, but is profoundly convinced that his sphere of action is and must always be of secondary importance.

The application of psychological methods to the cure of nervous disorders is to their application in education as the cure of consumption is to its prevention; and the latter is largely due to the efforts of those who understand something of the laws involved in its treatment, though it is not necessary that they should have been patients in a sanatorium. Nearly every town-dweller, though healthy, harbors the tubercle bacillus, and so every educationalist, though well adjusted, harbors repressions that are potentially harmful. Again, everyone engaged in the prevention of phthisis would profit from the hygienic measures that constitute his propaganda. So there is not a school-teacher or parent who would not profit in his or her own mental life from the principles of mental hygiene which this volume is meant to utilize. The work, too, of preventing tuberculosis is too vast to be left to sufferers from this disease alone; and so the applications of analytic psychology to the needs of the young are too urgent and too extensive to be committed to the few who have had a nervous breakdown or have undergone treatment. One anxious for the well-being of the new generation cannot prevent impatience with the obstructionists. Analytic views have, in fact, spread so rapidly in the last eighteen years that the reactionaries will soon be negligible.

But the most serious obstruction, as so often happens, comes from within, *viz.*, from the jealousy of pioneers and their followers who, while wishing to safeguard the movement, proclaim that no one can heal who has not himself been healed, that no one can initiate who has not been initiated, and that no one can preach who has not been ordained. These should heed the lesson of the New Testament: "Master, we saw one casting out devils in Thy name, and we forbade him because he followed not us." What is

needed is that teachers should recognize that no creed or initiation can qualify, but that the vision which allows us to perceive the child's needs and difficulties and potentialities is what is mainly needed; and this work is designed in some measure to contribute to this clearer vision on the part of teachers who are yet content to be learners. The chapters, after the Introduction, are: authority and suggestibility, reality and phantasy, emotional development: the boy, emotional development: the girl, the unconscious motive, mental mechanisms, dream symbolism, the herd instinct and the herd ideal, and educational methods.

Percy Bysshe Shelley: An Introduction to the Study of Characters. By THOMAS VERNER MOORE. Edited by E. A. Pace of the Catholic University. Psychological Monographs Series.

The author is not entirely satisfied with the analysis or definition of character given by Thorndike and his pupils, although more or less in sympathy, and taking his departure from them. He recognizes that the traits generally shown have not yet been, and perhaps cannot be, analyzed into their lowest terms so as to make them universal and everywhere acceptable; and he thinks therefore it is somewhat premature to attempt classification, but believes this must wait for the data of the kind he here undertakes. He assumes that poetry is, in general, a cryptogram to which the biography of the individual supplies a key.

Shelley, he believes, has some of the traits of dementia praecox as interpreted by Kraepelin and even Jung. Perhaps every man has his characteristic trend—praecox, manic-depressive, hysterical, etc. He takes recourse to his own conception of psychotaxes, which constitute the most characteristic elements of personality, determining how a man will act in emergencies, and thinks they may be classified into those which present no solution and those that provoke the defence-reaction and the compensations and sublimations. He is in sympathy with Adler's imaginary life-goals.

Shelley's chief attitude is one of revolt, and this was determined very largely by his bitter negative attitude toward his father, who abjured him although he was his eldest son. This made him sensitive to, and ready to find tyranny in, almost all the aspects of life. But wisdom and justice warring against tyranny were not enough for his romantic heart. He had to have someone who could understand his aspirations in their details; and so he turned first to Harriet and then, when she was utterly unsatisfactory, to Mary, with whom he led an almost gypsy life. His nature was a strange blend of physical and ideal, but he could ardently hold to nothing that did not involve conflict with the world about him, and in this his praecox disposition was most apparent. He was in conflict not only with his father and the masters at Eton but also with his schoolmates, who fagged him unmercifully, while his masters always misunderstood him.

The author has evidently carefully studied each of the several biographies of Shelley and his poems, and makes a pretty clear case that the latter express sentiments and experiences which were actually lived in a very vital way. Prometheus Unbound was Shelley himself; Jupiter was the tyrant of the world against whom he was in Satanic revolt.

The positive drive in Shelley's life was to find a woman who both loved and understood him, but none of them realized his ideal. His yearning for sympathy, stimulated of course by the bitter criticism to which he was subjected, gave him his patheticism, which appears in overdrawn pictures both of his sufferings and of the abuses to which he was subjected. This appears not only in his poetry, but apparently bred delusions in his soul, as of the insult and attack in Pisa by an officer who only knew his name.

The Psychology of Medicine. By T. W. MITCHELL, M.D. London, Methuen and Co. pp. 187.

Although this little work is designed mainly for beginners, there are chapters in it which will interest adepts as well. It is characterized by all the lucidity and impartiality for which the author is widely known. He begins with the theory of dissociation in hysteria and hypnosis, and these subjects are treated historically. Then follows repression, discussed from a very general point of view, and here and in most of the rest of this work he follows, in the main, psychoanalytic lines. Psychoanalysis is both a body of doctrine and a method of treatment, and is based mainly upon the theory of the unconscious and the neuroses. He discusses fully enough the differences of the sub-, co-, and un-conscious and the subliminal in their relations to one another. Perhaps the chapter on the neuroses, hitherto one of the most obscure themes, will be, on the whole, most helpful to all readers.

The book is by no means partisan, but treats sympathetically the work of the post-Freudians,—Jung, Adler, Silberer, and Maeder. This breadth of view would be more worthy of note in an American psychoanalyst, but he well and wisely insists that Freud's work is the foundation for all subsequent analytic doctrines. It is essentially a compilation, condensed and impartial, and the author makes no attempt to add to the views discussed or even to state very definitely his own opinions.

The Homosexual Neurosis. By WILHELM STEKEL. Tr. by J. S. Van Teslaar. Boston, Badger, 1922. pp. 322.

The first portion of this work was put into English by the same translator under the title *Bisexual Love.* The third part, entitled *Autoerotism,* is soon to appear, so that this is really the middle one of three volumes. One gathers pretty well from it, however, the author's point of view. He first treats the various relations of the homosexual to the other sex with an excellent epitome of views upon the subject. The second chapter deals with the rôle of the father and other members of the family. Then follow chapters on its relations to jealousy and of the latter to paranoia, the relations to sadism, and a final chapter on the neurotic's inability to love. This volume is rich in allusions to the author's wide clinical experience, and some of these cases are given *in extenso.* Stekel is one of the most original of the Austrian savants in this field who, while working in absolute independence of Freud, has never ostensibly broken with the Master, who seems to be showing an ever more feudal spirit toward those he has inspired who have differed with him on any points he has deemed essential.

The Psychoanalytic Study of the Family. By J. C. FLÜGEL. London, International Psychoanalytic Press, 1921. pp. 259.

Starting with a quotation from Plato's Republic, Book IX, which might almost have been a summary of modern psychoanalysis, the author treats very fully the relation between parents and children, brothers and sisters, while that between husband and wife is but lightly touched upon because psychoanalysis has less that is new to contribute here. In fact sex and marriage cannot be understood by a preliminary knowledge of the nature of the bond that unites parent and child. The author does not discuss the size of families; and his book, especially in the early part, claims to be only a compilation or epitome (and it is an excellent one), while in the latter, practical part he is more original. Here he brings out very well the necessity of family love, but stresses rather more, we think, than the facts justify, although they do go a good way towards supporting his claim, the necessity of later revolt from the influence of parents in the interests of progress.

Some Applications of Psychoanalysis. By OSKAR PFISTER. London, Allen and Unwin, 1923. pp. 352.

It is the aim of this book to show what psychoanalysis is and how it may be applied to the various spheres of human activity. It does not pretend to be an exhaustive treatise, but only to introduce the reader to a method of investigation which has a great future before it, and which some think will cause a radical revolution of our whole cultural thought. After discussing psychoanalysis as a method, then as an art, and showing its sphere in peace and in war, the author treats it as a philosophy, as a method with children, and in its relations to missionary work, the last part of the book being perhaps, on the whole, the most original, for he believes that its right use would immensely accelerate the process of evangelizing the world. This is the tenth work of the author to be put into English and his fifth large book.

Psychoanalysis in the Service of Education. By OSKAR PFISTER. London, 1922. pp. 176.

This book is addressed to teachers by the man who has done more to apply Freudian principles to education and also to pastoral work than any other. He believes that the moral, intellectual, and religious life of the rising generation would be greatly improved by a judicious use of paidology. Today we can descend into the subliminal regions where the threads of destiny are spun by gray-haired fates. Our conquest here may be compared to that of the air. Even Stern, who is opposed to Freudian analysis for children, believes in an analysis of his own, which is essentially that of Freud without sex. Flournoy and Claparède and others have felt it necessary to instruct teachers in the possibilities here. The unconscious is the world of the spirits of the world. Abundant spiritual life is buried there and cries out for deliverance. Those whose eyes are open see endless miseries caused by repressions, which educators do not understand. The hearts of those who thus descend into the kingdom of the shades, and set free fettered souls, must be pure.

Die Besessenheit. By T. K. OESTERREICH. London, 1921. pp. 403.

This Tübingen professor of philosophy here gives us a monumental study of possession which may fairly be called encyclopaedic in its range. He gathers his data from classical, religious, anthropological sources with great diligence and fairness, discussing first the nature of the study of possession and then the distribution of its phenomena and their import for the psychology of religion. He finds spontaneous possession among savages, *e.g.*, the North Asiatic Shamanism, and in it all emphasizes the possibilities of autosuggestion and a need for more detailed psychological observation. He admits parapsychical and even parapsychophysical phenomena. But it is somewhat disappointing that he gives us no theory, although from some passages and from his discussion of the Piper case one would infer that he has some sympathy with the English Psychical Researchers. He contributes several very interesting observations of his own.

Introductory Lectures on Psychoanalysis. By S. FREUD. Authorized translation by Joan Riviere. With a preface by Ernest Jones. 1922. pp. 395.

Freud was evidently not satisfied with the American translation of this book, published nearly two years ago, although it was made by an American relative of his; so that here we have another rendering of it into

English. The work is interesting and covers a wider field than any of Freud's previous works, so that it is well adapted to be regarded as an introduction to the subject for beginners. It does not, however, attempt to cover the whole field of psychoanalysis, dealing only with the psychopathology of everyday life, dreams, and the neuroses. Freud begins in a way so elementary that those familiar with the system will perhaps be impatient and expect little from what follows, but, as one reviewer has remarked, he has several different styles, a condensed and a more elaborated and a purely esoteric style, and all these are found in this book, so that the latter part of it takes the reader into the very profoundest depths of his *Tiefenpsychologie*.

The Psychology of Misconduct, Vice, and Crime. By BERNARD HOLLANDER, M.D. London, Allen and Unwin, 1922. pp. 220.

The author has had twenty-five years of experience as a practitioner with nervous and mental diseases, and records the results of his reflections. There are relatively few references to literature, and it would seem a little as though he were what the Germans call an autodidact or self-taught. He treats the bases and causes of misconduct, drink and drugs, acquisitiveness, ill-temper, violence, morbid suggestion, cunning, theft, fear of consequences, sex perversions, the egoist, autocrat, the morally weak-minded, responsibility, treatment of moral failings. The author's style is vigorous, lucid, and simple to a degree, and there is a freshness about his impressions, which are first-hand, that lifts the book distinctly above the commonplace. It is throughout sane, broadminded, and should be of assistance not only to physicians but also to laymen interested in abnormalities.

The Psychology of Everyday Life. By JAMES DREVER. 3rd edition. London, Methuen, 1922. pp. 164.

This title, as a perusal of the fourteen chapters shows, is to some slight extent a misnomer, or at least may be misleading to those who assume that the author is a follower of Freud. To be sure, the attention he gives to the Freudian work that might be characterized under this name is sympathetic, but it is very slight. He does discuss appetites, instincts, emotions, moods, sentiments, play, imagination, tastes and flavors, sound and colors, perception, illusions and hallucinations, etc., in a way that on the whole does justify his title. The book is untechnical and is well suited to the general reader. The author is influenced more by Lewes' psychology of everyday life than by Freud, although his acknowledgments are to McDougall, Stout, Titchener, James, etc., as well as to the Freudians.

Taboo and Genetics. By M. M. KNIGHT, I. L. PETERS and PHYLLIS BLANCHARD. Moffat, Yard & Co., New York, 1920. pp. 301.

This book attempts to summarize our natural knowledge of the biology, zoology and psychology of the foundations of the family institution and at the same time to suggest a starting-point for a more rational system of social control in this field. Knight treats of the new biology and the sex-problem in society in five chapters. In part II Peters discusses the institutionalized sex-taboo in four chapters. In part III Blanchard discusses the sex-problem in the light of modern psychology. It is an original, stimulating, and interesting work.

Psychanalysis in the Classroom. By GEORGE H. GREEN. New York, G. P. Putnam's Sons, 1922. pp. 272.

This book, with a brief introductory note by McDougall, was made in England and seems a misnomer, for it is not psychanalysis in the classroom but rather outside the classroom; it deals with dreams, play, instinct, re-

pression, sex, etc. The phenomena dealt with are mainly normal, and the author thinks that it is not only possible but also desirable to apply psychanalysis to all children. It is difficult to see how teachers can make much use of such studies, but parents might do so with profit to themselves and to their children. The book contains many illustrative incidents which are treated in psychoanalytic fashion. It is not entirely obvious why McDougall takes the opportunity in his preface to take a fling at behaviorism. Nor is it obvious why the writer finds it necessary to condemn so much of psychanalysis, especially sexual implications, while he repeatedly in the book seems to recognize all claims of the apostles of the libido as to its dominance.

Studies in Dreams. By Mrs. MARY ARNOLD-FORSTER. With an introduction by Morton Prince. London, Allen and Unwin, 1921. pp. 188.

We have here an essentially new theory of dreams by a cultivated woman, herself a remarkable dreamer, who tells us much of the state of the faculties, reason, memory, will, imagination, during the progress of the dream. Her theory (chap. 9) certainly deserves very careful attention. She is well-versed in the literature on dreams, including the Freudian theory, which she accepts only to a small degree. It is probable that no one theory of dreams could explain them all, for dream life is far more varied than normal waking life. This author delights in her dream life and finds as enchanting recreation in it after the cares of the day as a novel-reader. In some, we find symbolisms; and in others, none. Many dreams are repeated; others, not. Especially interesting is her claim that dream life may be cultivated, and this can be done because it is not all unconscious. Although normal dreams are of value for the light they throw on abnormal psychology, it is still truer that the varied data of the latter and the larger field are subject to experimentation and give an understanding of the former, reversing thus the Freudian psychology. The author has not only forbidden unpleasant dreams but has taught herself to fly. She is her own self-analyst, another contradiction of Freud.

Psychoanalysis and Behavior. By ANDRÉ TRIDON. New York, Alfred Knopf, 1921. pp. 354.

The author first discusses the organism, *i.e.*, body and mind, and then takes up in succession the problems of childhood, progress and regression, sleep and dreams, problems of sex, psychoanalytic treatment, and finally, and most interestingly of all, the four schools of psychoanalysis, those of (1) Freud the pioneer, (2) Jung and the Zürich school, (3) Adler and individual psychology, (4) Kempf and dynamic mechanism.

The Kingdom of Evils. By E. E. SOUTHARD, and MARY C. JARRETT. With an introduction by Dr. R. C. Cabot, and a Note upon Legal Entanglement by Roscoe Pound. New York, Macmillan, 1922. pp. 708.

This is a volume dealing largely with cases that came before the Boston Psychopathic Hospital as they were reported by Southard, before his death, and by his associates. It is a storehouse of interesting information, with a breezy and characteristic introduction by R. C. Cabot. The contents is grouped in four books: (1) the three spheres of social work; (2) the five major points of evil in the *regnum malorum*; (3) eleven minor groups of mental disease; (4) epicrisis. The large page and the small print of much of the volume enable the authors in this seven-hundred-page work to pack away a vast body of interesting material.

Suggestion and Autosuggestion. By CHAS. BAUDOUIN. London, Allen & Unwin, Ltd., 1922. pp. 228.

Part I deals with spontaneous suggestion in ten chapters, part II with reflective suggestion in eight chapters, and part III with induced suggestion in seven chapters. This volume is an exposition of the results of the studies of suggestion by the School of Nancy founded by Liébault and Bernheim and now since greatly developed by Coué. Whereas a voluntary act is one of which consciousness is aware, the mechanism of a suggested act remains subconscious. There are three types of mental activity—instinct, habit, and will. Suggestion cannot be reduced to any of these but makes a fourth, quite as important as any of them. With use, it reacquires a prestige which, according to Delboeuf, was originally possessed in an earlier stage of evolution. In those days a living being was fully aware of all that went on within. Owing to the division of labor his attention was increasingly directed outwards, and the supervision of the inner world was left to the subconscious.

Suggestion reestablishes the supervision, reconquers a lost realm, but is no detriment to subsequent gains. The teleology of the will is conscious; that of suggestion, unconscious. In the affective sphere, Freud finds affective complexes of sentiments which are unconscious which we control by making conscious. In the representative sphere, Bergson founds everything upon the distinction between intelligence and intuition. The latter slumbers, but has much knowledge of the greatest import. By the study of intuition we solve problems which intellect cannot. The Nancy School deploys in the native sphere rather than toward theory. Autosuggestion is to the will what the complex is to the sentiment, what intuition is to intelligence. Psychoanalysis, intuitionism, and the new School of Nancy contribute to the same general movement. We now live on the surface of things and do not arrive at intuition. The will is the normal mode of acting on matter without. Suggestion is the normal mode of acting on ourselves. We are masters of nature externally only. Inwardly we are nature's slaves. We have become too objective in our habits of thought. We have forgotten our own personality, and now or never we must know ourselves.

BOOK REVIEWS

The Psychology of the Special Senses and their Functional Disorders. By ARTHUR F. HURST. London, Henry Frowde, Hodder and Stoughton, 1920. pp. x., 123.

This book presents the Croonian Lectures delivered before the Royal College of Physicians in June 1920. Apparently the author has gathered together the most significant of his clinical cases, largely from his war experience. The material is frankly clinical rather than experimental. In the opening chapters he reviews the standpoint of Gendrin and Charcot on the physical and mental stigmata of hysteria. The physical symptoms, we recall, are chiefly cutaneous and pharyngeal anaesthesia and retraction of the field of vision. Babinski had already thrown doubt on the alleged importance of cutaneous insensibility and the retraction of the visual field as stable symptoms. Hurst has confirmed this doubt and has added valuable material on the anaesthesia of the mucous membranes, the pharynx and allied sensibilities.

The principal theses of the author are that the sensory processes are dynamic and that hysteria is essentially subjective in nature. The sensory processes are "active" and "require an effort of the will just as much as walking;" that is, to feel, to see, or to hear one has to pay attention, one has to set up a receptive attitude. The symptoms of hysteria are, therefore, really due to "suggestion" and "autosuggestion." These now popular terms describe for the author the production of Charcot's stigmata as well as the method of removing them. "*Hysteria is thus a condition in which the symptoms are present which have resulted from suggestion and are curable by psychotherapy*" (11: author's italics).

The first chapters concern themselves with the presentation of cases and the discussion of cutaneous and pharyngeal anaesthesia. There is a short chapter on "hysterical pain", which one wishes were more complete. The later chapters concern themselves with deafness and blindness in the author's patients.

Numerous cases are given to show that anaesthesia in the manner of Charcot and Janet is not a permanent feature of the patient's condition. The hysteria which Hurst found in his so-called shell-shock individuals was invariably nothing but the persistence of the physical symptoms after the organic lesions had healed. The clumsy diagnosis of the physician set up in the patient the attitude of belief in his permanent insensibility in the sensory fields affected in the original instance. Often the soldier's autosuggestion enhanced this notion. "When a man is temporarily deafened by a loud noise or by some recoverable disease, he finds that he is unable to hear, however much he tries, and consequently after a time he gives up trying. That is to say, he ceases to listen, and when the cause of the deafness at last disappears, he has become so convinced that he cannot hear that he makes no further attempt to listen"(63). The writer shows that the hysterical deafness reaches below the level of the auditory-motor reflexes, a result which confirms the experiments of Sherrington and Forbes on decerebrate cats. "In our cases of completely bilateral deafness the auditory-motor reflex was completely absent, but returned simultaneously with the restoration of hearing as a result of psychotherapy" (66); and further, "we found that the only test upon which almost complete reliance can be placed in the diagnosis of absolute hysterical deafness from absolute organic deafness is the presence of normal vestibular reactions in the former and

their loss in the latter" (72). Hurst admits that slight evidence has been brought forward to show that organic deafness may occur without involving the vestibular canals; but he thinks that by and large his tests are adequate, and that such material as is reported by Fraser does not invalidate his position. He produced some remarkable cures, by his psychotherapeutic methods, which rather confirm his theory.

Hysterical blindness in his cases is confined very largely to three forms: amblyopia, blepharospasm and ptosis. Only the first, in the proper sense, pertains to vision directly; the other two are due to muscular troubles in organs accessory to vision. The patients studied for blindness had frequently suffered severe shelling, resulting in many cases in sand and debris being thrown into their eyes. This produced irritation and temporary loss of sight. The symptoms persisted after the original cause had disappeared.

Following his clinical cases, at this point Hurst introduces the interesting historical cure of La Dame Stapart in the 18th century by a crude method of autosuggestion at the shrine of a former miracle worker.

The final section of the book gives some useful data on the unreliability of the stigma of retraction of vision in hysteria. Babinski in 1909 presented evidence to disprove the notion of Janet that retraction of the visual field is "the emblem of hysterical sensibility in general." As with cutaneous and pharyngeal anaesthesia, and with pain and hysterical deafness, the principal difficulty in vision lies in the awkward diagnoses of physicians.

After reading these data of Hurst's, one questions, however, whether all the cases he dealt with are true hystericals. Is so-called shell-shock, from which his patients were suffering, to be compared with the psychoneurosis known in ordinary psychiatry as hysteria? The author assumes, further, that by attention and suggestion the patient may reinstate the blocked synaptic connections and thus reëstablish the sensory pathways. Increased resistance at the synapses, then, becomes for him the cause for the inability to use the special senses, formerly cut off by organic lesion. He nowhere discusses the likelihood of the use of vicarious or compensatory reflex arcs. Bury and Dana, who are convinced that such paralyses as Hurst describes are not functional at all but physical, that is, biochemical, call attention to "relapses" in similar cases. At any rate, the controversy over the real basis of the symptoms goes on. We must await further results, in which the experimentalist should co-operate with the clinician. We need better control, and more careful description of the curative methods described in the broad terms: suggestion, autosuggestion, will and attention.

Clark University KIMBALL YOUNG

A Study of American Intelligence. By CARL C. BRIGHAM. With foreword by Robert M. Yerkes. Princeton, Princeton University Press, 1923. pp. XXV, 210.

The subject-matter of this book reminds one of the recent theories and contentions of Stoddard. Its main purpose is an attempted explanation of the fact that the best scores in army tests were made by the immigrants who had been here longest. Brigham's thesis is that "the average intelligence of our own immigrants is declining" (197). This condition he considers due to two factors: (*a*) the immigration to the United States of "the Alpine and Mediterranean races has increased," while that of the Nordic races has materially decreased; and (*b*) the "representatives of the Alpine and Mediterranean races in our own immigration are undoubtedly inferior to the representatives of the Nordic race" (197). In order to expound and prove the above thesis, Brigham divides his text into two parts.

In Part I. he first presents a graphic analysis and justification of the army tests. While there is nothing new in this section of the book, it has value in that it is a brief, comprehensive presentation and justification of

the army tests and combination scale,—which, or course, have been considered before in more elaborate and detailed form in *Memoir XV* and the *Army Mental Tests*. Brigham arrives at the same conclusion that the authors of these publications arrived at. He points out that "after weighing all the evidence, it would seem that we are justified in ignoring most of the arm-chair criticisms of this[alpha] test and in accepting the experimental evidence tending to show that the test was a fairly good one" (31). And again: "the reader . . . will be convinced that most of the tests [eight alpha, the Stanford Binet scale, and tests 4, 5, 6 and 7, of the beta] used were satisfactory" (59). Incidentally, he points out some matters of general interest: for instance, that the statement one frequently hears "that the tests proved that the average citizen of this country has a mental age equivalent to that of a child of thirteen . . .[is] ridiculous . . . The term 'mental age' is bad scientific slang for a total score" (56). He also notes that the prevalence of the notion that the army tests were "speed" tests, handicapping the ones who were "slow but sure," is not at all warranted; when the tests were given in "double time" the "absolute score" was higher, "but the relative position of the members of the group would be about the same" (11). It is a pity that in this connection Brigham failed to point out also that in "double time" a larger number of members of the group would score a place in class "A."

Secondly, Brigham deduces from his study of the army tests that they are tests which measure *innate* intelligence. He argues, as others have done before him, not only that the tests in general give an excellent criterion which can be used and has been used with success in the selection of army officers (15) but also that they are tests of *native* intelligence (100). As to the usefulness of the tests we must agree, and pay our humble compliment to the work of the army psychologists; but for the definite conclusion that the tests measure *innate* intelligence there are not yet enough data. From the so-called crucial and objective tests we may infer only that the tests are tests of intelligence, not necessarily that they are tests of *native* intelligence. It is to be expected that men in more advanced economic positions should score higher than men who are economically subordinate; but to say that that is necessarily because of a difference in *native* intelligence is surely unwarranted.

Part II. of Brigham's book concerns itself in the main with the proof of the principal thesis concerning immigration outlined at the beginning of this review. In the first two sections, Brigham studies the army score of a representative group of white and negro army draft, noting, of course, that the negro is found to be inferior in intelligence. Beginning with the third section, and continuing to the end of the book, he analyzes the army test score of the white draft. He finds that the native white draft ranks higher than the foreign draft; that is, "of the native born, 74% exceed the average of the foreign born" (85). He finds further that the score of the foreign born draft represents a uniform upward tendency in ratio to the years of residence in the United States (89). That fact, of course, has been well known, and many explanations have been offered. Brigham attempts to ground the usual explanations. In considering the "important problem . . . of interpreting the fact of increase of intelligence test scores with increasing years of residence," he asks: "Does our curve [the score of the foreign born white drafts in ratio to residence] . . . [1] represent the growth of intelligence with length of residence, [2] does it represent an error in the method of measuring intelligence, or . . . [3] does it show the gradually decreasing intelligence of the more recent immigrants examined in the army" (100)?

The first possibility is disposed of by Brigham with the statement that the score is a measure of *native* intelligence (100). This conclusion, as we have pointed out, is uncertain.

The second possibility is disposed of at considerable length. If there is an error in the methods of measuring intelligence, it is likely to be found in the alpha test, because of the implied use of the English language; but that is not the case, Brigham thinks, because "if the increase in the average score on the combined scale from 11.41 to 13.82 were due to the language and educational factor [Brigham usually means formal education], then the gain should come from alpha and not from beta, for alpha involves language and (indirectly) education, and beta does not. We actually find that the gain from each type of examination is about the same. This indicates, then", that the real explanation lies in differences in *native* intelligence (102). It is unfortunate that Brigham did not consider seriously the possibility that the language difficulty for the non-English speaking people in the alpha tests was perhaps paralleled by a difficulty of adjustment to the methods of the beta tests; and that this might account for the uniform rise of the score in both types of tests. On this point he writes, in another connection: "If the tests used included some mysterious type of situation that was 'typically American', we are indeed fortunate, for this is America, and the purpose of our inquiry is that of obtaining a measure of the character of our immigration. Inability to respond to a 'typically American' situation is obviously an undesirable trait" (96). We may fairly ask, at this point, whether this "typically American" trait is also an *inborn* trait?

The author therefore thinks that he is forced to the last possibility, that is, that "the curve indicates a gradual deterioration in the class of immigrants examined in the army, who came to this country in each succeeding five year period since 1902" (111). With this proposition in mind, he tries to find and to explain the reason underlying this deterioration. His explanation is that the immigration has radically changed since 1886 from a predominance of the Nordic race to that of the Alpine and, especially, Mediterranean races (112). He finds further, in an analysis of the mental score obtained by the various nationalities making up the foreign draft, that the Nordic races lead, the Alpine follow, and the Mediterraneans come last. He finds the following relative standing of the various nativity groups to prevail: England, Holland, Scotland, Germany, Denmark, (United States,) Canada, Sweden, Norway, Belgium, Ireland, Austria, Turkey, Greece, Russia, Italy, Poland. From these facts he derives the final conclusion that the representatives of the Nordic races which immigrate to the United States, in decreasing numbers every year, are really of higher *native* intelligence than the representative immigrants of the other two races of Europe. From this it follows that the "future blended American [made up of four racial elements,—the Nordic, Alpine, Mediterranean, and the Negro] will be less intelligent than the present native American . . . The deterioration of American intelligence is not inevitable, however, if public action can be aroused to prevent it" (205, 210).

To the statement that immigration is declining in intelligence no one can take exception. But whether, as Brigham argues, this is due to lack of *native* intelligence in the new immigrant is a much more doubtful matter. It is true that in previous years the United States has attracted from Europe, in part at least, a higher class of people,—people who came across the Atlantic to find freedom, salvation, and adventure. It is perhaps true that the Nordic peoples have sent, and are still sending, a greater proportion of their middle class than the Mediterraneans have. But to say that the middle class possesses a higher *native* intelligence than the peasantry of Russia and Italy is quite a different thing. Is it not as reasonable to suppose, instead, that the immigrants of past years were more easily assimilated and trained, and that for two reasons: first, because there were fewer of them who came and there were greater opportunities offered them when they had arrived; and secondly, because the training and environment of most of the immigrants (Nordic) who came to us in those days were more like our own? Another reason why the Nordic immigrant as a whole scored relatively higher than

the Mediterranean, irrespective of years of residence, may lie in the fact that the Nordic people in our country are better organized and established than the Mediterraneans, and thus have had more chance for development.

Whether or not, however, Brigham's warning of approaching degeneration is quite warranted, he has written a book which few psychologists will fail to find useful.

Northwestern University A. J. SNOW

Spiritism and Commonsense. By C. M. DE HEREDIA, S. J. New York, P. J. Kenedy & Sons, Publishers to the Holy Apostolic See. 1922. pp. xv, 220, and 7 half-tone cuts.

Spiritism and the Cult of the Dead in Antiquity. By LEWIS BAYLES PATON. New York, The Macmillan Co., 1921. pp. ix, 325.

The first of these works is a popular treatise written by a Jesuit father for Catholic laymen. Its material is briefly and interestingly set forth and its author writes with a wide knowledge of the current literature of the subject—*vide* the fourteen page bibliography of works in English, French, Spanish and Italian with which the book closes—and, except upon matters with reference to which the Church has already spoken definitely, his open-mindedness and scientific poise are admirable.

Accepting a minute residue of the alleged "psychic" phenomena as possibly well authenticated, he shows, nevertheless, in chapters on the origin of spiritism and on the psychology of the observer, of the medium and of the séance, the nidus of fraud in which modern "spiritualism" arose and the atmosphere of deception and credulity in which it still flourishes. As to the possibilities of deception in such matters Father de Heredia speaks as an expert, for he himself is an accomplished magician (as a boy he took lessons from Herrmann) and is able at desire to produce under test conditions most of the striking phenomena of the professional mediums: the materializations, the spirit photographs, the "ectoplasm", the levitations, and the rest.

In later chapters he points out clearly the logical gulf between the acceptance of "psychic" phenomena as actualities and the proof that they are the work of discarnate spirits, and considers in succession the diabolic theory, the natural theory (telepathy), and the spiritistic theory, accepting the first as admissible in rare instances and the second as probable, and rejecting the third. Two further chapters, on Spiritism as a Religion and on Spiritism and Morals, together with an Epilogue, complete the main part of the book. Five appendices deal with special topics.

The second work is a timely contribution to the anthropology of the belief in life after death by the Professor of Old Testament Exegesis and Criticism in the Hartford Theological Seminary. The author treats in thirteen chapters of the status of this belief in primitive religion, in ancient China, among the Indo-Europeans, in Egypt, and among the early Semites, Babylonians and Assyrians, and finally of the changes that it underwent among the Hebrews from their entrance into Canaan to New Testament times through the gradual triumph of Jahvistic conceptions,—on which changes Dr. Paton speaks, of course, with especial authority. A final chapter is devoted to an exposition of the teaching of Jesus as to personal immortality and to a defense of the historicity of his resurrection: which last section, in the reviewer's judgment, is not germane to the rest of the study and might have been omitted with advantage. The book is excellently indexed.

Though Dr. Paton reveals in his preface a certain hospitality toward the results of "psychic research", going in this respect at least as far as Father de Heredia and much further than most psychologists, the scholarly

and readable chapters of the body of the work, with their fully documented detail, have evidently not been affected by this extraneous opinion. These chapters demonstrate with cumulative force that the chief phenomena of spiritism have not only appeared at all times and among all peoples, but have given rise also to pretty much the same sort of response on the part of those who were credulous with regard to them. They furnish to psychology a problem of the highest interest, toward the solution of which not only anthropology and "psychic research" but also psychiatry and social psychology have in all probability significant contributions to make.

E. C. S.

A Nursery School Experiment. By HARRIET M. JOHNSON and CARMEN S. REUBEN. New York, Bureau of Education Experiments, 1922. pp. 81.

The interest in this bulletin lies in the problems it sets rather than in the results. In the introduction by Lucy Sprague Mitchell we have two answers to the question: "Why do we want such young children?" The first answer is that the experimenters wish to study the educational value of environmental factors; the second, that they wish to secure data "concerning children's growth,—growth of every sort that is measurable or observable." This bulletin returns a partial answer to the first of these questions in particular, and incidentally gives some facts relative to the second.

Cornell University L. B. HOISINGTON

Psychologie des Gefühlslebens. By GUSTAV STÖRRING. Zweite Auflage. Verlag von Friedrich Cohen in Bonn, 1922. pp. vii, 289.

In a review of Störring's "Psychopathologie," published in the "Zeitschrift für Philosophie" (Bd. 120), Meumann wrote: "In der That würde ich lebhaft wünschen dass der Verfasser in einer neuen Auflage des Werkes manche Anomalien der Gefühle.....beträchtlich ausführlicher behandelte." As the dedication of the "Gefühlsleben" runs "Meinem Freunde Ernst Meumann in treuem Gedenken," the work is surely a pious response to Meumann's wish and, perhaps, to the extent to which Meumann busied himself with the psychology of feeling, a memorial.

In this work Störring brings together and systematizes previous investigations and discussions of feelings, emotions, and organic sensations, —taking them up first in their general aspect, in their relation to their bodily accompaniments, and last in their special aspect, as aesthetic, moral and religious feelings. Between these comes a section on the play of states of feeling in various "psychical functions" such as attention, will, and the flow of ideas.

It is impossible, within the limits of this notice, to do more than glance at the rich content of the book. Perhaps a detailed examination is the less called for as there is no essential departure from the conclusions set forth in the various investigations from which the book is derived. In fact, one meets many familiar paragraphs, especially from the "Psychopathologie."

Störring is a good Wundtian,—which means that, while accepting Wundt's fundamental principles of psychological investigation, he by no means accepts all of Wundt's theories and findings.

He rejects the tridimensional theory of the feelings; feelings of tension are for him vaguely localized sensations of tension. No more is excitation a feeling; supposing that it accompanies a rise in the excitability of the cortex as a result of continuous work, Störring regards it as

obscure consciousness of a facilitation in psychical processes. In this respect it would seem somewhat akin to the 'Anregung' in the Kräpelin Heidelberg investigations. It is assuredly a different thing from the 'Erregung' mentioned by Störring in reference to Kelcher's experiments, which is more of the nature of irritation.

Pathological findings have brought Störring to consider fixation through an act of will as the essential mark of attention, the driving force of the fixation coming from a feeling element of will-action. In this connection *concentration* is spoken of as a form of attention which, as applied to a feeling, increases its intensity. But attention, as usually directed to a feeling, decreases its intensity. A closer analysis of this state of concentration is much to be desired. Zoneff and Meumann also mention it and also without analysis (Phil. Stud., 18).

No more than Wundt will Störring hear of states of 'doppeltes Bewusstsein' and of 'subconscious selves'; for explanation of the states bearing these names he calls into action the play of organic sensations. Naturally he is antipathetic towards the Freudian structure: the action of the suppressed idea is a matter of fact, but unconscious reflection and unconscious volition are matters of fancy. In fact Störring is that *rara avis*, a psychiatrist who believes in basing explanation of abnormal mental phenomena on established principles of psychology and in explaining the unknown by the known and not by the mysterious.

But besides being psychiatrist Störring is a philosopher and doctor of medicine and psychologist of the Leipzig laboratory. In the exercise of his several vocations he has gathered a large amount of 'material' and it is grateful and comforting to find that in evaluating this material he abides in the straight and narrow path of scientific psychology in a domain where wandering in the by-ways of extemporary psychologizing seems to be a seductive diversion.

In bringing together and arranging in logical form the outcome of his investigations in the psychology of feeling, Störring has rendered a valuable service to the science.

FRANK ANGELL

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NOTES

Visual Intensity

I have been accustomed to treat the question of visual intensity by way of the colour pyramid. I do not think that the following account contains any new idea. Since, however, it leads to results which seem to me to be at least not inconsistent with those which Stumpf arrives at in his recent monograph[1], I judge that a brief Note may be worthy of publication.

Let us begin by removing grey from the colour pyramid. We then have a geometrical figure of the same sort as the smell prism. We have a hollow shell, every point of whose surface is occupied by some visual quality, at its representative best.

Every such quality is, at the same time, a point upon an intensive line. Let us suppose that these lines are added to the figure. We then have, as it were, a hollow pincushion stuck all over with pins, whose length represents the length of the intensive series belonging to the various qualities. The pins are not necessarily driven in to the same distance, since the best representative quality need not correspond throughout with a particular degree of intensity. Nor, in theory, need the pins tend all alike toward the center of the pincushion; the pin that contains an apical quality might, *e.g.*, lie (aside from the intensity of this one quality) altogether outside the figure.

So far, the colour pyramid may be paralleled by a geometrical construction of the qualitative-intensive data of any other department of sense. So far, visual intensity is psychologically given, just as intensities of touch or taste or smell are psychologically given. Only, in fact, we have the complication of grey,—that anomalous quality which has no qualitative pair, no degrees of intensity[2], no part of its own in adaptation or after-image or contrast.

Grey goes, then, to the center of our hollow shell. Immediately, all the pins are, so to speak, psychologically polarised. All now have their points converged to the center of the figure, and all have their heads flush with its boundary planes. For the introduction of grey means that all qualities at all intensities are (psychophysically!) mixed with grey; and this mixture means that all the intensive visual lines tail off toward grey as a common meeting point, and that the quality of maximal intensity becomes the best representative quality. Psychologically, of course, we have lost both our original intensities and our original qualities. The originally intensive lines have been changed to qualitative; and the original qualities themselves have all been clouded or diluted.

I suggest, now, that the transformed and polarised intensive lines may have a twofold psychological status. So long as they are included in the original color pyramid, they are qualitative, and can be nothing else. But let us change our attitude, and—to make the change apparent—let us reconstruct the pyramid in terms of these lines alone. We get a star or sunburst, whose nucleus is grey, and whose rays extend from the nucleus to all points on the surface of the former pyramid. I suggest that the lines of this new figure may be called pro-intensive series. They are not pure intensive series; but they represent, none the less, our nearest possible approach to visual intensities.

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[1] C. Stumpf, *Die Attribute der Gesichtsempfindungen*, 1917.

[2] G. E. Müller, Zur Psychophysik der Gesichtsempfindungen, *Zts. f. Psych. u. Physiol. d. Sinnesorg.*, x., 1896, 31; xiv., 1897, 60 ff.

It remains a question whether the pro-intensive series can be handled, in direct psychological observation and without recourse to perceptive analogy, as intensive. At any rate, it seems plausible to suppose that grey stands as the minimum of visual intensity at large (though I confess I am not quite sure how far psychophysical considerations are involved in this plausibility); that white, since the grey-white line is the longest of all the lines in the figure, stands as the maximum of visual intensity at large; and that the colours familiar in daylight vision possess under ordinary circumstances an approximately constant degree of intensity within the middle range. These are Stumpf's conclusions. It may also be, as Stumpf seems to think, that at any rate certain pro-intensive lines, over a limited portion of their extent, lend themselves to rigorously intensive observation; but on this point further experiments are wanted.

E. B. T.

Wundt's Address at Speyer, 1861

There is no more remarkable incident in the history of experimental psychology than Wundt's attempt, by way of a single crucial experiment, to overturn the whole Herbartian psychology (cf. *Beiträge z. Theorie d. Sinneswahrnehmung*, 1862, xxi., note). One of my colleagues will presently publish a full account of the episode. Meantime I can dispose of a document in the case.

In 1861 Wundt gave an address before the *Naturforscherversammlung* in Speyer "über die persönliche Differenz zwischen Gesichts- und Gehörsbeobachtungen mit Rücksicht auf die astronomischen Durchgangsbeobachtungen" (recorded in our bibliography as 1861 (6); this Journal, xxi., 1910, 603). Through the courtesy of Mr. P. Anders, of the firm Gustav Fock of Leipzig, I have received a transcript of this item. The report in the meeting's *Tageblatt* is a bare summary. Since its publication may save trouble to other enquirers, I reprint it here.

"Herr Dr. *Wundt* aus Heidelberg sprach dann über [die] persönliche Differenz, welche zwischen Gesichts- und Gehörsbeobachtung eintritt. Diese Grösse hält er für absolut, aber positiv resp. negativ, weil die verschiedenen Menschen zuerst sehen und dann hören oder umgekehrt, daher erklärt sich der Redner den Unterschied zwischen *Bessels* Beobachtungen und denen anderer, welche, nicht wie *Bessel*, zuerst gehört und dann gesehen haben.

"Herr Professor *Argelander* erklärte, dass er erst sehe und dann höre. Bei der Beobachtung vor dem Faden sei aber die Aufmerksamkeit stärker als die nach dem Faden, daher man bei letzterer den Stern häufig zwei Mal zu sehen glaubt, woher 0, 1 - 0, 15 Sek. Differenz entstehen. Dann beurtheilte er die *Bessel*' sche Beobachtungsweise, welche sich gerade in dieser Beziehung als fehlerhaft herausgestellt hat, bei der Vergleichung des Längenuntershieds zwischen Königsberg und Berlin, welcher später vermittelst des Telegraphen bestimmt wurde. Er erwähnte dann einer der Zeit proportional vor sich gegangenen Vergrösserung der Differenz zwischen ihm und Struve. Zwischen diese Zeit fällt eine schwere Krankheit von Struve, welche keinen Einfluss gehabt hat, und fordert jüngere Astronomen auf, ähnliche Vergleichungen anzustellen."

E. B. T.

Urban's Tables Once More

H. H. Long's correction of Urban's Tables (the value of 2 p for $p = .73$ should be 0.7551 instead of 0.7541), which was reported in this Journal (33, 1922, 303) by E. G. Boring, not only necessitates the changes in Rich's Checking Tables (this Journal, 29, 1918, 120) in the values when $p = .73$ and $x = +2$ and $x = -2$, which were pointed out by Boring, but also changes in the values when $p = .27$ and $x = +2$ and $x = -2$.

The changes in Rich's Tables which have to be made because of this latest discovered error in Urban's Tables are: the value for $p=.73$ and $x=+2$ should be 7.2317 instead of 7.2307; for $p=.73$ and $x=-2$ should be 2.2363[1] instead of 2.2373; for $p=.27$ and $x=+2$ should be 2.9665 instead of 2.9675; and for $p=.27$ and $x=-2$ should be 2.9915 instead of 2.9905.

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THE SCANDINAVIAN SCIENTIFIC REVIEW

We have received the first number of a new periodical, entitled "Scandinavian Scientific Review: Contributions to Philosophy, Psychology and the Science of Education by Northern Scientists." The Review, which is written throughout in English, is edited by Dr. M. L. Reymert, formerly a fellow in psychology at the University of Iowa, with the assistance (among others) of Professors A. Aall and A. Grotenfelt, and Dr. R. H. Pedersen. Of the five articles that make up the number we mention A. Aall, "Alfred Lehmann: In Memoriam;" R. H. Pedersen, "Weight of School-Children in the Summer Time;" C Spearman and J. Flügel, "The Present Status of Psychological Science in Great Britain and Ireland." The Review hopes presently to issue a monograph series, and also to extend its scope to include other sciences than those named on its title-page.

ARKIV FÖR PSYCHOLOGI OCH PEDAGOGIK

This quarterly journal is a continuation and enlargement of the Swedish "Psyke" and "Svensk arkiv för pedagogik," both of which have ceased publication. It will contain contributions to psychology and education in the Scandinavian languages. The editors are S. Alrutz and B. Hammer (Upsala) and A. Herrlin (Lund). The first double number of vol. I. appeared in April, 1922.

THE JAPANESE JOURNAL OF PSYCHOLOGY

This new psychological magazine, whose first number is dated January, 1923, is edited by M. Matsumoto, H. Hayami and K. Masuda, all of Tokio. The contents of the number are: M. Matsumoto, On the Methods of Psychology; K. Masuda, What is Psychology? The Study of the "Vorgesetztes Erleben" in its Subjective and Objective Aspects; K. Imada, The Relation between Thought-processes and Verbal Ideas; and T. Oguma, The Contents of Abnormal Psychology. The titles are promising; but the articles must, alas! remain unread by western psychologists.

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[1]This value was mistakenly reported by Boring. *Cf.* G. J. Rich, this JOURNAL, 33, 1922, 450.